THE FEBRUARY SCIENTIFIC MONTHLY

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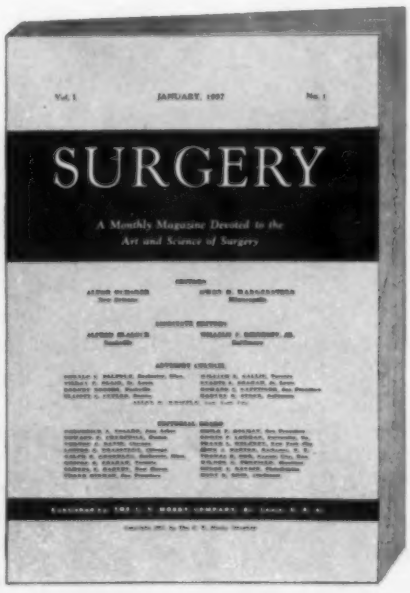

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FEBRUARY, 1937

VISITING VENEZUELA

By Dr. FRANK D. KERN

PROFESSOR OF BOTANY AND DEAN OF THE GRADUATE SCHOOL, THE PENNSYLVANIA STATE COLLEGE

HISTORICALLY Venezuela is an interesting place but not well known to most North Americans. Doubtless only the best students of American history remember that Columbus discovered the northeastern peninsula of Venezuela known as Paria on his third voyage in 1498. The next year what is now the whole Venezuelan coast was traversed by Ojeda and Amerigo Vespucci. Gradually conquered and settled by Spaniards, the region remained under their control for three hundred years. In the middle of the eighteenth century the colonists rose in revolt but were suppressed. Successful insurrections, fostered largely by Simon Bolivar, the great South American patriot and Liberator, brought about independence in the early part of the nineteenth century. Bolivar has been referred to frequently as the Washington of South America. As a consequence the name of Bolivar is encountered on every hand in Venezuela to-day. The Spanish pronunciation of his name is *Boleevar*. Every city and town has a "plaza Bolivar." In Caracas, the city of his birth, a monument has been raised in his memory; his remains were removed to Caracas in 1842; and the centenary of his birth was celebrated at Caracas in 1883. The unit of money in Venezuela is known as the "bolivar." The only other name at all comparable in the history and development of the country is that of General Juan Vicente Gomez, president for the past quarter century.

From 1819 to 1830 what is now Venezuela was a part of the republic of Colombia. Between 1830 and 1908 there were many civil wars and changes of constitution. Till 1864 Venezuela was made up of provinces, but since then has been a federal republic. It is perhaps not well known that the official designation is "The United States of Venezuela." There are twenty states and the federal district, in which the capital, Caracas, is located. In area Venezuela is just about equal to Texas and New Mexico combined, but its population of three million is less than three fourths that of those two states.

Since 1908, when General Gomez took over the reins of government, the tranquility of this nation has been outstanding in South American history, which has been marred by conspiracies, revolutions, dissentions, civil and international wars. Through all this Venezuela has enjoyed peace, quietude and progress under General Gomez. Cyrus Norman Clark, recently reviewing twenty-five years (1908–1934) under Gomez, wrote:

> While every other republic of the southern continent has been harassed by one or another form of political unrest, Venezuela alone has been undisturbed, free to pursue the development of her internal resources and foreign commerce. Her sister nations have struggled with

STATUE OF SIMON BOLIVAR

IN THE CENTER OF PLAZA BOLIVAR, CARACAS.

unbalanced budgets or have defaulted, wholly or in part, in meeting their national indebtedness; while Venezuela has exhibited, year after year, an ample surplus of revenue, has liquidated her entire foreign debt, has reduced her internal debt to less than twenty-three million bolivars, and at the end of the last fiscal year possessed a treasury balance of nearly sixty-one million bolivars![1]

In 1908 Venezuela had a foreign trade of one hundred and thirty-seven million bolivars. By 1931 that trade had reached the sum of one thousand and twelve million. During the same period the national revenue increased from fifty million bolivars to one hundred and eighty-five million bolivars. Agriculture, manufacturing and natural resources have been developed. Coffee is the most important agricultural product. Cacao is next to coffee in importance. These two crops do not compete, as coffee is grown in the higher altitudes while cacao is grown in the lowlands. Considerable sugar-cane is grown, but much remains to be done to improve its quality and yield. Stock-raising has received attention in the great grassy plains of the Orinoco Valley, but has not begun to reach the proportions to which it could be developed. A government Agricultural Bank has been of great assistance to planters and breeders. The principal manufacturing enterprises are cotton textiles, sugar, paper and cement. Without doubt, the greatest single factor in the prosperity of Venezuela has been the discovery and development of the petroleum industry. The chief petroleum fields are in the Caribbean region around the shores of Lake Maracaibo. This great body of inland water is in reality

[1] Before the devaluation of the dollar, five bolivars equaled one dollar gold.

"COLORADITO," *POLYLEPIS SERICEA*

THE TREE THAT REACHES THE HIGHEST ALTITUDES IN THE ANDES OF MERIDA (THIS PICTURE TAKEN AT 3,500 METERS). THREE INTERESTING FUNGI WERE COLLECTED ON THIS SPECIMEN.

DIGGING OUT WILD POTATOES IN THE PARAMO ABOVE TIMOTES

CHANCELLOR CHARDON (CENTER) AND COMPANIONS. BOTH TUBERS AND SEEDS HAVE BEEN OBTAINED FOR BREEDING AND EXPERIMENTATION.

not a lake but an arm of the sea with a mouth which is a strait five miles wide. Its waters are always fresh, for they come down from the high Andes in such quantities that they push out the salt water from the sea. The oil fields are so close to the lake in some places that the derricks and tanks stand on piles a quarter of a mile from shore and can be reached only in launches.

The petroleum industry in Venezuela has been developed by foreign interests, principally British and American. Friends of the Venezuelan administration claim that the legal enactment under which foreign capital has operated there is a model for similar legislation. Whether that claim is justified may be difficult to determine, but it surely is true that mutually satisfactory arrangements have prevailed. The foreign companies have been dealt with liberally and at the same time the interests of the country have been conserved and the public treasury has had ample returns. What might happen if the strict governmental control should lessen is a matter for conjecture only. Although one gains the impression that the present oil development is very large, it is probably true that it doesn't represent a half of the possibilities.

With Professor R. A. Toro, of the University of Puerto Rico, I visited Venezuela during April and May, 1934, making a study of plant diseases and collecting fungous parasites of both cultivated and wild plants. I was especially interested in the plant rusts (Uredinales) and my colleague in the black fungi (Pyrenomycetes). Our activities were in continuation of botanical explorations begun and carried on under the direction of Chancellor C. E. Chardon, of the University of Puerto Rico, himself an enthusiastic collector and an excellent student of fungi. A 350-page volume with 33 plates of illustrations

TWIN PEAKS IN THE ANDES OF VENEZUELA
NAMED IN HONOR OF THE EARLY BOTANISTS, HUMBOLDT AND BONPLAND.

entitled "Mycological Explorations of Venezuela" by Chardon and Toro, in collaboration with various specialists, published by the University of Puerto Rico in 1934, is the best evidence of the type of work being done. That volume is founded chiefly on previous explorations made by Chancellor Chardon and Professor Toro in 1932 and reports 667 species of fungi, of which 92 are new to science. Our collections of 1934 have not yet been reported, but preliminary studies indicate that we have several more new species. And of scarcely less interest are the collections which add to our knowledge of existing species. To find a specimen which extends the geographic distribution of a known species or one which adds to our knowledge of its life-history is a real thrill to the collector. On this expedition we collected several species of rust of more than usual scientific interest and several of economic interest. The rust on alfalfa was taken for the first time in northern South America, having been previously reported from Argentina and southern Brazil. We found a rust doing considerable damage to a planting of Guatemala grass. We are reminded of the appearance of this rust in Puerto Rico shortly after Guatemala grass was established as a crop there. It seems likely that the rust will follow the cultivation of this grass. This appears to be the first report of the disease from South America.

Of course, the real pioneer botanical exploration of South America began long ago. The work of Joseph Jussieu, which began in 1747, deserves first mention. He did not touch the region now occupied by Venezuela. Unfortunately he brought back only a few specimens. Humboldt and Bonpland in strenuous explorations at the beginning of the nineteenth century were in the Venezuelan area, and one is reminded of them to-day by two high Andean peaks named in their honor. They explored jungles, forests and mountains and brought back a rich collection which became the foundation of our knowledge of the flora of northwestern South America. Much later we can not overlook the fine work

of Dr. Henri Pittier, who is still living and working near Caracas. We found his book, "Manual de Plantas Usuales de Venezuela," published in 1926, most helpful in determining the hosts of some of our parasitic fungi. We visited Dr. Pittier, and he was kind enough to examine and identify some of our collections. It is impossible to refer here to many of the botanical pioneers in South America. There is a considerable list and they did wonderful work, but the task of working out the tropical flora there was tremendous and it is far from complete to-day. The expanse of tropical area in South America exceeds the United States in extent. I am glad to pay a tribute to the work of the systematic phanerogamic botanists, both to those who did the pioneer work and to those who are carrying on the splendid work to-day in the field and in the great botanical centers of the world. The student of fungi must frequently call on the phanerogamic botanists to classify and name his host plants, and he has, therefore, a real interest in the activities of phanerogamic botanists. I find it necessary frequently to call on these systematists for aid, and I can not be too enthusiastic in expressing my appreciation of their cooperation. They often get imperfect specimens and the cheerful and effective way in which they help in solving these puzzles adds materially to the mycological results.

Mycological studies in Venezuela were initiated by Dr. Adolfo Ernst, who went there in 1861. His devotion to the development of science and education in the country of his adoption earned for him a distinguished standing. The first systematic account of the fungi of Venezuela is the classical work of Patouillard and Gaillard entitled "Champignons du Venezuela et principalement de la region du Haut Orenoque recoltes en 1887 par M. A. Gaillard." Its pioneer nature can be appreciated when it is known that 45 per cent. of the 278 species were new. From the time of Patouillard and Gaillard, the French botanists, Venezuelan

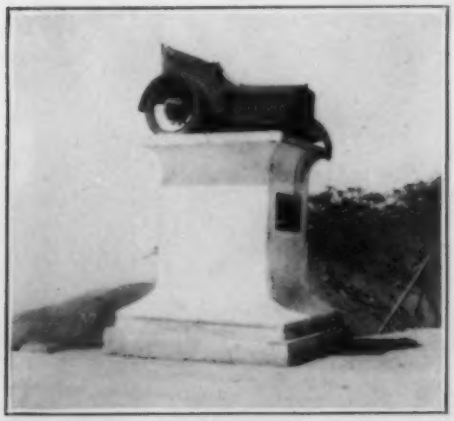
THE UNIVERSITY IN CARACAS

THE BUILDING WAS FIRST USED AS A MONASTERY AND DATES BACK TO THE EIGHTEENTH CENTURY.

STATUE OF LIBERTY

ON ONE OF THE PRINCIPAL STREETS OF CARACAS.

mycology did not show much advance until the visit of the eminent German mycologist, Dr. H. Sydow, in 1927. In his paper "Fungi Venezuelani," published in *Annales Mycologici*, of which he is the editor, Sydow reported 316 species



A ROADSIDE WARNING

TO THE EFFECT THAT RECKLESS DRIVING IS DANGEROUS. ERECTED BY THE ROTARY CLUB OF CARACAS BETWEEN LAGUIRA AND CARACAS.

of which 35 per cent. are new. The next mycological work of importance is that of Chardon, Toro and their colleagues, referred to in a foregoing paragraph.

During our recent trip we naturally kept to the field as much as possible, industriously and minutely examining all vegetation in quest of these more or less elusive parasites, yet we found time to make some general observations.

We stayed for several weeks in and about Caracas, which is a most delightful city. It enjoys a perpetual June climate, partly accounted for by its altitude of 3,000 feet. As one sees it to-day there is an interesting mixture of the old and the new. The business sections are old and are characterized by narrow streets and by large numbers of small establishments. Especially noticeable and numerous are the shops of the tailor, druggist and the barber. Larger stores with modern show windows are beginning to come in. The houses in the older sections are all on exactly the same plan. They are built next to the sidewalk with a narrow passage leading to an open *patio*, from which the rooms are entered. The street windows are all provided with window seats and are protected by a projecting iron framework, which extends ten or twelve inches from the wall. This permits an excellent opportunity to remain at home and yet to observe what is going on in the street. The sidewalks are just about wide enough for two people, except for the telephone poles. It is necessary to go single file past the poles. A bird's-eye view of Caracas from the top of one of the surrounding mountains was suggestive at that distance of an enormous tile-yard, since the buildings are uniformly low and the roofs almost exclusively tile. In the newer suburbs the plans are thoroughly up-to-date, with wide streets, beautiful lawns, gorgeous plantings and gardens and houses that are not only modern but often what I would call modernistic. There are several beautiful country clubs, some of

them with excellent golf courses. Another modern feature about Caracas is the number and quality of its automobiles. Possibly the narrowness of the streets by impeding traffic may give an erroneous impression of the number of cars, but certainly there is nothing to discount their fine quality and appearance. Commenting one day to a friend on the fact that all the cars looked so clean and shiny, I was told that there was an ordinance requiring owners to keep their cars washed and polished. I suspect I was being joked, but nevertheless the effect was there as if it might be true. Any notes about Caracas would be incomplete without reference to street noises. They are of two main sorts, one the incessant and largely unnecessary honking of automobile horns, and the other the cries of the vendors of lottery tickets. The striking thing in connection with the latter is the persistence in repeatedly announcing the numbers they have for sale, evidently with the belief that their particular numbers may turn out to be favorites if sufficiently emphasized.

There are two North Americans honored with statues in Caracas—George Washington and Henry Clay. An amusing incident is related with regard to the statue of Washington. It has its setting at the time it was necessary to cut down some trees in the Plaza Washington to make way for the building of a theater. It is said that one night a wag went to the plaza and attempted to console the Father of his country on the removal of his trees, but receiving no answer from the statue, turned away woefully with the remark, "Anyhow, I suppose you realize it serves you right; for it was you, was it not, who cut down your father's tree?"

AVENIDA LA PAZ—AVENUE OF PEACE
IN ONE OF THE NEWER SUBURBS OF CARACAS.

Splendid concrete highways radiate in several directions from Caracas. The main traveled one leads to Maracay, a distance of about sixty miles, where General Gomez spends most of his time on his extensive and favorite estate, "Las Delicias." Here in Maracay every pos-

sible travel comfort may be enjoyed in a large and beautiful hotel. As much can not be said for all the towns which we visited. In the fine hotel in Maracay we had the interesting experience of having an employee mix three languages in addressing a short sentence to us. Arriving at our room rather late, we found that clean linen had not been put on our beds. In response to our summons there came a porter who agreed that we were right and who set about to perform the functions of a chambermaid. It is fair to explain in advance that his native language was German, that my friend bantered him in Spanish, and that I chimed in frequently with English. Amid some confusion after he had put on the fresh sheets and finished making the beds he dashed out with the sudden exclamation, "En seguida I bring zwei pillows." On another occasion at a small hotel in an interior town we began to think that we would have to sleep in the hammocks hanging in the room, so slow were they in making up the beds. We were finally provided for in a satisfactory manner, but our chauffeur was not so lucky. The next day we had much fun laughing at his story of how he had to pay an additional fee for each piece of bedding, much to his disgust. One hotel had first-, second- and third-class rooms, reminding one of a steamship. After taking a first-class room we found it a tax on our imaginations as to what second and third class might be like. At another time in a room with two beds I selected a large bed with one of the most gorgeous hand-crocheted spreads I have ever seen, leaving to my companion a plain small bed. At the moment I felt a bit selfish, but I wished to have the experience of sleeping in a bed so grand. My friend had plenty of fun at my expense when it turned out that my grandeur was in appearance only, the mattress resting on boards, whereas his little cot had excellent springs.

"HOTEL JARDIN" IN MARACAY

THE PICTURE SHOWS A PART OF THE *patio*.

The central and really important feature of our visit to Venezuela was an expedition to the Andes Mountains. We had intended to spend considerable time in the great llanos region, which constitutes about a fourth of the country and lies between the Orinoco and the northern mountains. The llanos are great undulating grass plains, broken here and there by low plateaus and belts of forest. They have been investigated

A GLACIER IN THE HIGHER PART OF THE VENEZUELAN CORDILLERA

very little botanically. At the time we were there it was toward the end of a long dry season, and we found the plant life so much dried up as to make explorations unprofitable.

Hence after a few days in the western llanos we decided to change our plans and cross over to the great transandine highway, which is a part of the international road between Caracas, Venezuela,

A VIEW ON THE TRANSANDINE HIGHWAY

KNOWN AS THE GRAN CARRETERA DE LOS ANDES, ILLUSTRATING THE NATURE OF A SHELF ROAD CARVED OUT OF THE MOUNTAIN SIDE.

ANOTHER VIEW ON THE GRAN CARRETERA DE LOS ANDES

and Bogota, Colombia. This highway is a magnificent engineering accomplishment and represents a tremendous amount of labor. They tell you there that it is the highest automobile highway in the world. It winds around those great mountains, finally reaching an altitude of 4,080 meters at the highest pass. It seems impossible to present in a word picture any adequate account of the curves and grades. There are many thrills for the traveler. It was nothing uncommon to learn that a village nestled in a valley below was one that had been passed through two or three hours before. The route seemed to have been selected with great engineering expertness and the grading done with accuracy and carefulness. The roadbed needs improving, and the tourist who has been used to guard rails and precautions to prevent rockslides finds much to be desired. Fortunately, traffic is light, and equally fortunate is it that neither course nor surface lend themselves to fast driving. A passenger in an automobile accustomed to speedometer recordings in miles is pleasantly misled by the recordings in kilometers. I am sure that a reading of 30 on the speedometer has a psychological effect different than its equivalent of 18 would have. Many hours through the mountains our speedometer was registering not over 30 kilometers or about 18 miles per hour.

A LANDSLIDE ON THE HIGHWAY

CARS ARE WAITING WHILE WORKMEN CLEAR AWAY THE ROCKS.

After we reached the transandine

DON FELIX RAMIREZ CARILLO, WITH HIS WIFE

IN FRONT OF HIS HOUSE IN CHACHOPO. HE GUIDED THE PARTY OF CHANCELLOR CHARDON TO THE WILD POTATO COUNTRY. THE COBBLESTONE STREET IS TYPICAL OF MANY VILLAGES.

A WHEAT FIELD IN THE ANDES OF MERIDA

ALTITUDE 2,600 METERS. PARAMO COUNTRY ABOVE, IN THE SKY LINE.

VEGETATION IN A TYPICAL PARAMO REGION

THE CONSPICUOUS WOOLLY PLANT IS A COMPOSITE KNOWN TO BOTANISTS AS *Espeletia corymbosa*.

highway we found that we had some real desert to traverse before we began to ascend the high mountains. Our road must have taken us at least a hundred miles through this region, which is known as the Desert of Lara. The xerophytic flora is characterized by columnar cacti and low spiny trees of the mimosa family. A mistletoe was found growing on some of the trees, and on it we collected an interesting rust. For a considerable distance in the desert the road followed the dry bed of what is a stream during the rainy season. Fortunately the rainy season is short, for it very successfully interrupts traffic. It was just before crossing this desolate region that we were warned about "economic fever." An interest on our part in a malady with such a strange designation elicited the explanation that the name was appropriate because the disease was so quickly fatal that it precluded the possibility of calling a doctor.

Without doubt the most interesting experience in roaming up and down these towering mountains is that of passing from zone to zone as if one were darting back and forth between the tropics and the arctics. Both one's feelings and the vegetation reveal the similarity in effects produced by altitude and latitude. The kaleidoscopic view from the tropics to snow here in the shadow of the equator is impressive. Starting with the heat and flora of the tropical jungle, it seems more or less an imperceptible change to the subtropical zone with its citrus, coffee, bananas and sugar cane. The temperate zone is more clearly defined and the presence of wheat, barley and potato fields, apple and peach trees attract the immediate attention of the northerner. The wild flora in the temperate zone is characterized by plants with thick leaves. It seems as if composites predominate. Ferns are abundant. At the high altitude of about 3,000 meters there begins what is known to the Spanish-speaking people of this region as the "paramo" country. Here the most characteristic plant is a large

A GROUP OF INDIANS IN FRONT OF THEIR HUT
WHICH IS BUILT OF STONE AND STRAW. PARAMO DE TIMOTES, ALTITUDE 3,700 METERS.

somewhat bushy composite, the leaves and flowers of which are covered with a dense woolly coating. It belongs to the genus *Espeletia*. I am sorry I have no common name for it. In Venezuela it is known as *frailejon*. Gentians, Verbenas, Lupines, Thoroughworts and Cudweeds are in evidence, but the flora becomes thinner as the altitude increases. Above 4,000 meters there are no trees. A common shrub looking somewhat coniferous is a St. Johnswort. The leaves of this give off a disagreeable odor, especially while drying. The *Espeletia* is quite at home above the tree line.

These higher altitudes known as the "paramos" are very sparsely inhabited. Most of the people we saw were Indians, who seemed to be eking out a miserable existence in poor houses with stone walls and thatched roofs. Neither crops nor animals thrive, and why these people stay there at all seems a mystery. In the temperate and subtropical zones the houses are adobe with tile or metal roofs. The climate is mild, farming is successful, and life is pleasant.

Dr. Chardon, in a bulletin of the Pan-American Union published in 1933, has discussed the life zones of the Andes of Venezuela at some length. He recognizes four altitudinal life zones, tropical, subtropical, temperate and paramos, which, he says, correspond to *teirra caliente, tierra templada, tierra fria* and *paramo* of other authors. These last four are the names generally in use in Venezuela, Colombia and Ecuador. Dr. Chardon calls attention to the fact that on the transandean road between Motatan, with an altitude of 340 meters and luxuriant tropical life, and the Paramo de Mucuchies, with an altitude of more than 4,000 meters and a temperature close to freezing, all the four life zones can be passed through within a five-hour journey.

I quote the following from Chardon's account. "As far back as Humboldt and Bonpland's trip to the New World

ONE OF THE TEMPERATE ZONE VALLEYS

AT THE ENTRANCE TO THE GREAT CORDILLERA. THE PICTURESQUE VILLAGE OF TIMOTES IN THE DISTANCE. THE ELEVATION HERE IS ABOUT 2,000 METERS. AN ADOBE HOUSE WITH TILE ROOF IN THE FOREGROUND.

(1799–1804) the Andean Cordillera was known to be divided into distinct life zones. These scientists made accurate observations on the altitudinal distribution of plant life, especially in the description of the paramos. The Colombian Francisco Jose de Caldas added to our knowledge of altitudinal distribution and its effect on Andean agriculture. Dr. Alfredo Jahn, of Caracas, in his geographical studies of the Andes of Merida ("La cordillera venezolana de los Andes," 1912) and in a recent paper read before the Sociedad Venezolana de Ciencias Naturales ("Los paramos venezolanos," 1931) has made extensive observations and discusses specifically the paramos of the Venezuelan Andes. In Dr. Henri Pittier's flora (previously mentioned) appears a detailed treatment on altitudinal zones. Dr. Frank Chapman, in his bird studies of Colombia (Bulletin of the American Museum of Natural History, 1917), gives an excellent discussion of life zones of the Andes and their altitudinal distribution. His work is cited here as one of the most interesting contributions to Andean natural history."

Any story of travel in Venezuela which does not touch on the gates, chiefly at entrances to the cities but sometimes at state lines, would be woefully incomplete. The government wishes to know who is traveling and where and the records taken at these gates furnish this information. An automobile trip has frequent interruptions, while these gatekeepers check up on you. They record the names of all the passengers, the chauffeur, name and number of the car, and they do not forget to ask where you are from and where you are going. At most of the gates policemen in uniform

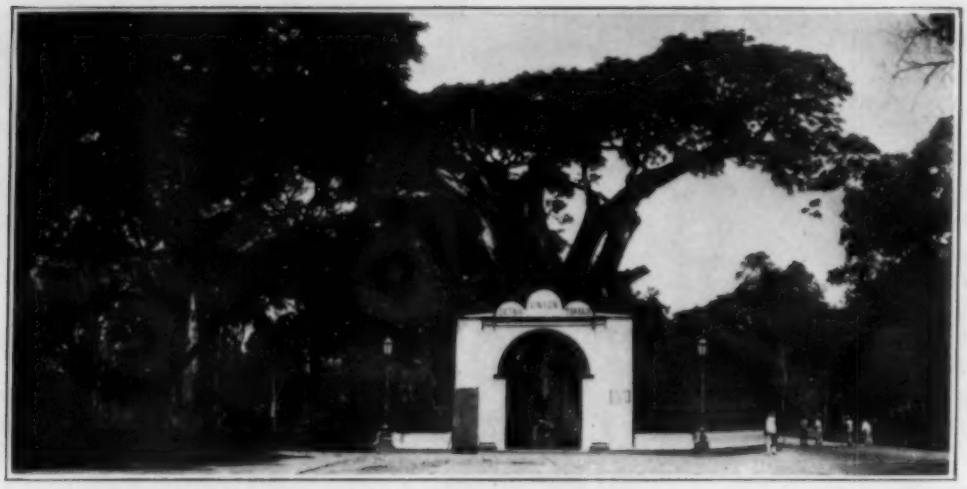
are in charge and in evidence, but sometimes only a boy is encountered. There seems to be a variation in the care and enforcement of the regulations. At some gates, especially at state boundaries, there is a tendency to search luggage. I state it in this way because if there is a rule to that effect it is often disregarded. At one place an officer told us that they really inspect the face more than the baggage and on that basis he knew we were honest and would make no inspection. It seemed more likely to us that something in the nature of our credentials influenced his decision to let us off so easy. At another gate we were required to wait a considerable time while a careful and complete record of our credentials was made. When we returned through this same place some days later, there was no formality and we were much amused by the instructions of the officer in charge to the gate keeper, "Let them pass, I know them very well."

Not long ago I saw a despatch in one of our papers to the effect that an automobile trip through Venezuela is anything but a joyride under present conditions and mention was made of the frequent stops and the ransacking of baggage by suspicious officials. It was our experience that a good deal depends on the attitude of the traveler. It was necessary to comply with regulations, but at no time was there anything unpleasant about what was required and everywhere the finest respect was paid to us and to our credentials. Sometimes we were told to go to the office of the *jefe civil* (might correspond to mayor) to register before going to a hotel. We always did it and we found all officials with whom we came in contact to be friendly and helpful. It is my idea that those who get into trouble in such places are the ones who think that they have the right to decide whether or not they will comply with regulations.

In an article on the progress of botanical exploration in South America (*Bulletin* of the Torrey Botanical Club, 1932) Dr. H. A. Gleason has called attention to the tendency that botanical explorers have shown to give way to their enthusiasm by collecting first in the region of their port of arrival and then when starting inland to follow established

ONE OF THE "REGISTRATION" GATES

THE SPANISH DESIGNATION IS *alcabala*. SOMETIMES INSTEAD OF ACTUAL GATES THERE ARE HEAVY CHAINS STRETCHED ACROSS THE ROAD FASTENED TO POSTS ON EITHER SIDE.



AN ANCIENT SAMAN TREE

THIS IS THE CHIEF SPECIES OF TREE USED FOR COFFEE SHADING.

roads or trade-routes for long distances. He points out that in this way the collecting of an explorer tends to overlap that of his predecessors. As I look back on our visit to Venezuela and particularly as I have here given an account of it, I believe my companion and I must plead guilty to having done what others have done. But Dr. Gleason has offered such a splendid defence of this natural tendency that I am tempted to quote. "This is not said in criticism," he writes. "Quite on the contrary, it is this repetition of work in the same territory that has been necessary to discover and collect the flora. Even yet that work is not absolutely complete, not even for small districts about the seaports, or for the frequented trade-routes. I venture to say that there is not in all tropical South America a tract of a hundred square miles with its flowering plants all known and listed. Finding the first half of a flora is easy. Finding the next forty per cent. is interesting, having the excitement of discovery without the necessity of too great exertion. Finding the last ten per cent. means long continued work, and an effort probably quite out of proportion to the interest or value of the results." Mycologically we have found so far only those things which are in the first half and so I think no apology is necessary for having taken routes that overlapped those of previous collectors.

For the success of our expedition thanks are due and are hereby accorded to numerous officials and friends. We are especially indebted to the President, and to the Ministers of Agriculture and Education, and members of their staffs for material aid. Several of the photographs included in this paper were made available through the kindness of Dr. Chardon. We were fortunate in coming in contact with a considerable number of people who encouraged us and gave us assistance of one sort or another. In this group is included the American ambassador. We wish it were possible to mention all these good people by name. We long for an opportunity to repeat these pleasant experiences.

THE PROMISE OF MODERN BOTANY FOR MAN'S WELFARE THROUGH PLANT PROTECTION

By Dr. E. C. STAKMAN

PROFESSOR OF PLANT PATHOLOGY, UNIVERSITY OF MINNESOTA; AND AGENT, UNITED STATES DEPARTMENT OF AGRICULTURE

BOTANY is the most important of all sciences, and plant pathology is one of its most essential branches. The statement is hyperbolic, of course, but it is not mere bombast. For man still is basically dependent on plants for subsistence. Plants are the sole creators of food and clothing materials; man and other animals are merely cultivators, transformers, processors or purveyors. When we consider the manifold additional uses of plants and plant products, including their service in building and conserving soil, in controlling floods and in increasing the esthetic enjoyment of life, the assertion in the first sentence could at least be defended in argument. At any rate it is well to remind ourselves occasionally that human subsistence is dependent on plants and that the number of people that can exist in the world is limited by its agricultural and aquicultural potentialities.

If plants are essential, it follows that their protection also is of paramount importance. Plants need protection against unfavorable soil and weather, against certain industrial by-products, against insect pests, against diseases caused by bacteria, fungi, eel-worms and other living organisms, and against filterable viruses. Crop plants may be severely damaged or commercially ruined by any of the above causes. It would be desirable but hardly feasible, within reasonable limits, to discuss the devastation caused by all these factors. Therefore, this discussion is restricted principally to plant diseases caused by plant parasites, except in so far as other factors must be taken into consideration in controlling them.

Ever since the dawn of recorded history plant diseases and insect pests have been among the greatest hazards in the production of crop plants, and they still are. It has been estimated that insect pests cost the people of the United States approximately a billion dollars annually, and plant diseases are about as costly. Plant pathologists and entomologists often are ridiculed for their allegedly extravagant estimates of damage caused by diseases and pests. But any one who has seen the devastation caused by epidemics of black stem rust of wheat and other small grains, by late blight of potatoes, by grasshoppers and by many other diseases and insects must be profoundly impressed with the magnitude of the financial losses, the tragic consequences to farmers and the far-reaching sociologic and even political implications of such crop catastrophes.

While the control of insect pests is not primarily a botanical problem, the relation of insects to many plant diseases is so important that a discussion of plant protection would not be complete without at least brief reference to it. Insects are the principal or only agents of dissemination and inoculation of many plant pathogens. In popular language, insects are tremendously important in spreading plant diseases. They are responsible for the spread of some of the

most destructive virus diseases, such as curly top of sugar-beets, aster yellows, sugar-cane mosaic, raspberry mosaic and many others. They also are largely or wholly responsible for the dissemination and inoculation of the bacteria or fungi that cause rots of potatoes and other vegetables, pear blight, the Dutch elm disease, wilt of cucumbers and related plants, and certain wood stains and rots of felled timber. So important are insects in connection with many plant diseases that disease control becomes a joint entomological and botanical problem. And the protection of crop plants against many insect pests themselves may well be accomplished by botanists through the breeding of resistant varieties. This method of controlling insects has not been used extensively, but there is evidence that it may become very important. Modern botany can promise much for the control of certain insects, but close cooperation between entomologists and botanists can promise still more.

How important is it to protect plants against diseases? Far more important than often is supposed. In a Mayo Foundation lecture several years ago Professor Whetzel pointed out that one third of the sweet potato crop of the United States is destroyed annually by diseases in the field or in the storage house, "one bean in every dozen, one apple in every seven, one peach in every eight, one bushel of Irish potatoes in every twelve, and one bushel of wheat in every ten, are destroyed annually by diseases in these crops." It is stated further that certain potato growers in Pennsylvania have increased acre yields of potatoes between 300 and 500 bushels over the average of the state by using disease-free seed and by spraying. In Minnesota, a number of years ago, yields of potatoes were increased 160 bushels an acre in demonstration plots through the use of disease-free seed. Yields of wheat have been almost doubled in experimental plots dusted with sulfur to control rust. The terrific stem rust epidemic of 1935 destroyed 12 per cent. of the wheat in Kansas, 15 per cent. of that in Nebraska, about 30 per cent. of that in South Dakota and about 60 per cent. of that in Minnesota and North Dakota. The total toll taken by rust in that one year was more than 125,000,000 bushels. The terrible devastation over thousands of square miles can scarcely be realized by any one who has not seen it himself. And epidemics of other diseases can be equally destructive over considerable areas. Thousands of acres of sugar beets are periodically abandoned because of the ravages of the curly-top disease; the chestnut blight has practically destroyed the chestnuts of the United States; and the Dutch elm disease is now menacing one of our finest shade trees. And these are only a few examples. Surely the protection of plants against diseases is of national concern.

Fundamental to crop protection is better crop adaptation. Just as nature has selected ecotypes of native plants, that is, strains particularly suited to certain soil and climatic conditions, just so must the crop ecologists or breeders select strains of crop plants that are particularly suited to certain environmental conditions. For drought, excessive summer heat and winter cold are grave hazards for crop plants. Their destructive effects are both direct and indirect. Several million acres of winter wheat are abandoned each year because of winter injury; about 15,000,000 acres, or one third of the total planted, having been abandoned in 1933; fruit trees are periodically killed or severely injured by cold, and during the drought years of 1933 and 1934 about a billion bushels of corn were destroyed by heat and drought. The weather can not be controlled, but its destructive effects can be reduced by developing adapted varieties and by the use of suitable cultural practices. Much has already been

accomplished. The substitution of Crimean wheats for Mediterranean types in the Southern Great Plains area has greatly reduced losses from unfavorable weather; the development of Minturki wheat has made winter-wheat growing safer on the northern fringe of the winter wheat region; the development of Ceres wheat has reduced somewhat heat and drought injury in the northern spring wheat region; the development of early maturing varieties of wheat and corn has reduced danger of damage by early frosts; and the development of stiff-straw varieties of small grains has reduced the danger of lodging on soils where it was common. But far more can and should be done. When we consider the terrific losses of recent years, the need for varieties still better adapted to resist unfavorable weather and soil conditions is apparent. And past progress indicates the possibility of greater accomplishment for the future. But superior varieties are not plucked out of a hat by tricks of legerdemain; they are the result of long and painstaking and laborious sorting and breeding and testing. The breeding of better adapted varieties is not a pastime for botanists in their spare moments. It requires time, labor, skill and adequate facilities.

Crop adaptation also is an aid in controlling some plant diseases caused by pathogens, particularly those that attack weakened plants. For unsuitable environment often predisposes plants to insidious but destructive diseases. Certain root rots of cereals, for example, are most destructive to plants weakened by unfavorable environment. Canker fungi and wood rots are likely to attack fruit trees that have suffered winter injury. There even is evidence that resistance to stem rust may be lowered when normally resistant wheat varieties are grown under environmental conditions to which they are unsuited. It is becoming increasingly evident that plant disease resistance is a variable character that is governed not only by genetic factors but also by environment; hence, the importance of having varieties well adapted to local soil and climatic conditions. While this is especially true of long-time perennial crops, it is true also of annual crops. Some European countries have long recognized the necessity of breeding locally adapted varieties, and there appears to be increasing appreciation of its importance in the United States. The desire for standardization, however, sometimes has resulted in growing varieties under conditions to which they are not suited. The breeder of locally adapted varieties can contribute significantly to plant protection.

A primary obligation of botanical science is to help promote what may be termed plant public health. And one way of accomplishing it is by preventing the promiscuous interchange of dangerous plant pathogens between regions and countries. This requires more knowledge, better techniques and better social attitudes. There are those, of course, who are opposed to plant quarantines on the ground that they are unnecessary or ineffective. But surely one can scarcely contemplate with serenity the devastation caused by chestnut blight, the total cost of citrus canker in Florida and other Gulf States, the economic importance of white pine blister rust, and the menace of the Dutch elm disease to our finest shade tree. These diseases are caused by pathogens that were brought into the country by man and could have been excluded by quarantines. And they are only a few of the total, to say nothing of introduced insect pests. When one reviews the history of many of the most destructive diseases, it is astounding to find how many of them have attained their present status through the activity of man himself. Many of them originally were restricted in importance and

geographic distribution; they could not have crossed natural barriers such as high mountain ranges, oceans, and even crop barriers, by natural means. But man, because of ignorance, apathy, carelessness or lack of foresight, did what nature could not do. He carried them to the far corners of the earth, where they often have persisted in their most pernicious form.

There is great potential danger in transporting propagative parts of plants from one part of the world to another, because a disease may be far more destructive in a new region than in one where it has long existed. The varieties grown in the new region may be far more susceptible than in the region where the disease has long been prevalent, or the weather and soil conditions may be more favorable. Because a disease is relatively unimportant in one region is no guarantee that it will be unimportant in all regions. The chestnut blight is far more destructive in North America than in its original home in the Orient because the American chestnut is very much more susceptible to it than oriental species. Citrus canker was not recognized as a destructive disease until it found extremely favorable conditions in its new home in Florida. Plant breeders repeatedly have developed disease-resistant varieties of crop plants, only to see them succumb to new parasitic races of the same pathogen which may have been introduced from other regions. The danger of introducing new strains of a pathogen is well illustrated by the fact that Anthony oats is fairly resistant to stem rust in the United States but completely susceptible in certain areas of Northern Europe, merely because different races of the stem rust fungus prevail there. There are similar situations with respect to other rusts, smuts and other destructive pathogens. Surely there is ample evidence that quarantines are desirable if they can be made effective.

Can quarantines be effective? The question can not be answered categorically. Obviously, the success of any quarantine will depend on the method of dissemination of the pathogen involved, on the nature of the barriers between the area where the disease occurs and the area to be protected and on the adequacy of the quarantine organization. Obviously there should be a scientific basis for every quarantine. This often has been lacking, and things have been done that need not have been done, and things have not been done that should have been done. The quarantine on Maine potatoes, about twenty years ago, because of powdery scab was costly, but it was neither effective nor necessary, because the disease already was present in one region that the quarantine was designed to protect and could not develop destructively in another because of unsuitable environment. It was a mistake. But whose fault was it? Not the quarantine organization's! Their action seemed justified on the basis of the available information, but there was not enough information available. The fault was with those who failed to provide for adequate plant disease surveys. And one reason clearly was the difficulty of overcoming the inertia or resistance of many people who refused to consider seriously what might happen but only what had happened. There is serious danger in having thoughts rooted too deeply in the immediate present or in the past.

Practical botanical science must project itself more into the future; and to do so it must not be too practical. For science surely has the obligation to forecast the future, not only to explain the past and present. Observations and researches should be made to find out where potential as well as present danger lurks. This can be done only by studying diseases over as wide a territory as is necessary, whether domestic or foreign. Domestic plant disease surveys are essential from a number of standpoints. Un-

fortunately, however, their value often is not recognized. It is pertinent to ask how plant pathologists can be expected to meet new situations, to interpret old ones and to prepare for future emergencies if they do not have opportunity to make adequate studies of diseases as they exist and the factors influencing their distribution and development. Plant disease survey studies, ecologic studies, are among the most important in the whole realm of plant protection, and yet we are often so myopic as not to appreciate their value and provide for their support.

In addition to domestic plant disease surveys, every country should interest itself in the diseases of other countries, especially those that grow the same kinds of crops. Information should be obtained regarding the methods of effective dissemination and the effects of the parasitic races of the foreign pathogen on the crop varieties and under environmental conditions similar to those of the country to be protected. Only when such information is available can the most adequate and intelligent quarantine action be taken. When we consider our own experience with chestnut blight, citrus canker, white pine blister rust and the Dutch elm disease, and the experience of many foreign countries with other diseases, we can scarcely avoid the conclusion that quarantines are essential; and, when we consider certain other cases, we must admit that unfortunate mistakes have been made in their application. The remedy, however, is not wholesale condemnation, but improvement through the results of research. And the research must precede practice, not lag behind it and do the second guessing.

The principles that apply to quarantines apply equally to eradication campaigns. Unfortunately, even some botanists still maintain a scornful or sceptical attitude toward large-scale eradication as a control measure. This was true also of many medical men when medical science entered the field of public health. If it is considered demeaning for botanists to assume leadership in plant public health measures, at least no one needs demean himself unless he chooses. Every scientist has a right to his scepticism, but he also has the obligation to study each situation honestly and thoroughly before pronouncing destructive *dicta ex cathedra*.

Some eradication campaigns have succeeded; others have failed. Some have been modified with respect to aim and scope; others were carried out as originally conceived. The difficulty is that emergency or public demand precipitated some of them before scientists were ready to supply the best techniques or accurately to predict the probable outcome. The chestnut blight eradication campaign failed; the citrus canker eradication campaign in Florida was conspicuously successful. The white pine blister rust campaign was not successful in completely eradicating the disease from the United States, but it has been eminently successful in controlling the disease for practical purposes in New England and the Lake States. The barberry eradication campaign has been successful in eliminating a tremendous number of annual local and regional epidemics of stem rust throughout the eradication area and in causing a downward trend in average annual rust losses. It has not resulted in complete elimination of general epidemics, but has reduced their frequency and is a practical control measure in the more eastern states of the area. Furthermore, the indirect benefits are almost incalculable. The existence of parasitic races of stem rust is one of the greatest handicaps to the development and maintenance of rust-resistant varieties. Varieties have been distributed as rust resistant, have retained their resistance for some time, and then have become susceptible because of the appearance of new or hitherto

unimportant parasitic races. Researches on the genetics of the stem rust fungus show that these new races are produced principally, if not almost exclusively, by hybridization on the common barberry. Almost 150 of these parasitic races are known, and there is definite evidence that new ones are produced and perpetuated through the agency of the barberry. From the standpoint alone of preventing the future development of new races and reducing the number now in existence, the barberry eradication campaign is worth-while. The most recent experience with parasitic races was in the crop season of 1935, when Ceres wheat, hitherto moderately to highly resistant to stem rust, succumbed to its ravages because of a combination of factors, including the prevalence of a relatively new physiologic race to which it is very susceptible under certain conditions. Is it too much to expect that some of the wider implications of such control measures as barberry eradication should be understood and appreciated?

Both white pine blister rust eradication and barberry eradication often are criticized because of changes in objective, method or, in some cases, unguarded statements regarding probable results. Unfortunately, it is impossible here to discuss fully the merits of the questions involved. However, both campaigns have paid for themselves many fold. Before condemning them because they do not eliminate the diseases completely, it would be well to picture what would have happened had they not been undertaken. Surely it should not be difficult to appreciate the value of a disease control measure that contributes significantly to the alleviation of a situation, even if it is not perfect or if it must be used in conjunction with other methods. The fact that techniques were modified or simplified and that the work could often have been done more economically and effectively had all necessary facts been available at the beginning merely strengthens the argument that research should not only accompany but also precede control measures. A highly significant contribution of modern botany to man's welfare will be to provide a technical service to accompany every control program and, particularly, to accumulate through research a reservoir of facts to be available as soon as necessity arises, not years afterward.

Botanical science can and should function far more effectively in future than in the past in protecting long-time pasture and forage crops against short and unprofitable life because of the inroads of insidious diseases. The increasing emphasis on grassland and forest creates new problems. Obviously, direct control of diseases often is difficult or impossible with such plants, and mistakes can not be rectified so easily as with annual crops. If a mistake is made with annual crops, it can be avoided the next year. But when pastures are established or forests planted, they constitute a long-time investment and changes to rectify mistakes are expensive. Therefore there must be adequate research in order that the mistakes may be prevented, that the most suitable kinds of plants be provided and proper cultural practices devised. Considerable is known about diseases of individual grasses and forage crops, but too little is known about the relative value of different strains, the relation of pure and mixed stands to the development of diseases, about the relation of soil type, site and fertilization to yields and longevity. And in many cases still less is known about the relative disease resistance of strains or biotypes within a species. At the Welsh Plant Breeding Station, Aberystwyth, the writer saw, a number of years ago, a large number of strains of orchard grass, *Dactylis glomerata*, which had been selected in the vicinity and propagated vegetatively. Not only did they differ greatly in growth

habit and other important characters, but some of them were virtually immune from yellow stripe rust, while others were completely susceptible. Obviously, such selection work and the incorporation of the results into agronomic practice is extremely valuable. The ecology and pathology of grasslands must be studied thoroughly if costly mistakes are to be avoided. They can and should be avoided, but botanical science must be put to work on the problems if they are.

Better protection of forest trees against diseases and deterioration due to wood rots is imperative if the land devoted to forests is to be used to best advantage. Necessarily, improvement must be attained through incorporation into silvicultural practice and forest management of the results of research. It is known, for example, that Armillaria root rot causes heavy damage to many of the most valuable tree species. But what is the relation of pure or mixed stands, density of stand, site and other factors to its development? This information should be utilized when the plantings are made. Likewise, the relation of similar factors to the development of canker diseases and wood rots must be learned and appropriate measures taken. In northern Europe it has been shown that larch canker is likely to be much more destructive in pure stands than in mixed ones, and this fact is taken advantage of in practice. With the increasing emphasis on managed forests in the United States, similar facts should be taken into consideration. Thinning operations, cutting methods and cutting cycles must be arranged with due regard to the protection and performance of permanent forests and woodlands. The art of growing healthy trees must be based more and more on scientific principles, but the scientist who discovers them and recommends their application also must learn more about the art of growing trees—not only in pots in the greenhouse, but in the woods. And he must learn more about the pathogens of the trees—not only on nutrient agar, but on the trees themselves. What is known, for example, about the relative resistance of biotypes within tree species? Almost nothing. But yet there are such biotypes, differing profoundly in growth characters and in disease resistance. On a private estate in Germany, for example, the writer recently saw plantings made from different seed lots of Scotch pine. The plants were growing under comparable conditions, but yet plants from different seed lots differed strikingly in rate of growth, growth habit and resistance to the leaf-cast disease. The trees in some of the plots were almost completely defoliated by the leaf-cast disease, while those in others were virtually immune. It was a beautiful demonstration of the existence of races or strains within a tree species. The importance of the application of genetic principles, particularly with respect to the selection of planting stocks, can hardly be overestimated. And even scientific breeding of trees is not a mere dream; a beginning has been made, and the possible value has been shown. The forests of the future can and will be far better than those of the present if science is given greater opportunity to function in their establishment and maintenance.

In the future far more will be done than in the past toward protecting crop plants against soil-borne diseases through cultural practices, including soil fertilization, time of sowing and rotation. The discovery that corn is predisposed to root and stalk rots by lack of sufficient phosphorus and potash; the discovery that early sowing of flax is likely to prevent serious injury by Fusarium wilt; the discovery that damage from fusarial head blight of wheat and barley can be greatly reduced by not sowing them on corn land suggest the possibilities of accomplishment in this direction. But there still is a vast field for exploration

and a rich reward in results of practical value in this phase of crop protection. How little is known, for example, about control of diseases caused by so polyphagous a species as *Rhizoctonia solani*? And yet there are definite indications that study of physiologic specialization in this species will yield results that can be applied with great profit in growing such important crops as sugar-beets, potatoes, tomatoes and other vegetables.

Investigations of antibiosis hold great potential promise for the future. It is known that some microorganisms have a tendency to inhibit or prevent the growth of others, including plant pathogens. Certain bacteria are known, for example, that prevent the development of smut fungi. Certain fungi are known that prevent the development of Rhizoctonia and other generalized parasites. It is one of the commonest observations in cultural work that fungi growing together may have no effect on each other, may stimulate each other, may be mutually antagonistic, or one may prevent the development of the other. This phenomenon has been studied in the past principally because of its scientific interest, but in future it should be studied also because of its potential practical importance. A beginning has been made, and promising results have been obtained. From these results it seems certain that the information obtained can be applied, at least on a small scale, to the protection of valuable ornamentals, and possibly of fruit trees. There even is promise that it can be used on a large scale in helping to devise cropping systems that will enable antibiotic organisms to function significantly in controlling such destructive and refractory diseases as potato scab, root rots of cereals and probably many others.

Chemical immunization of plants has been attempted so often and with such indifferent results that many plant scientists have concluded that it is impracticable. But recent results obtained by Hassebrouk in Germany show that it is definitely possible and possibly practicable.

Past progress in the control of diseases by fungicides, in cooperation with chemists, points the way to extensive progress in the future. The step from copper sulfate and formaldehyde to the best organic mercury dusts was scarcely dreamed of twenty-five years ago. And yet these most recent fungicides have largely eliminated the danger of seed injury, are much easier to apply, and they control certain diseases that resisted control entirely by the old fungicides. Then, too, there will be tremendous progress with respect to the specific applications of fungicides. The investigations at Cornell University showing that in some localities potato yields are increased greatly by increasing the proportion of copper sulfate to lime in the bordeaux spray, while the reverse is true in other localities, show how little we know and how much can be accomplished by precise investigations of fungicides and their effects. There is tremendous need for information regarding effects of different fungicides on different crop plants, on different pathogens and under different conditions. It is to be hoped that the "squirt gun days" of plant protection are on the way out; but they will linger on until there is wider appreciation of the necessity for investigations made on an adequate scale and with the required degree of precision.

There is great promise in the control of plant diseases through the development and use of resistant varieties. Indeed, some diseases can not be controlled economically by any other means. Flax wilt, wilt of peas, tomato wilt, asparagus rust, cabbage yellows and some rusts of cereals are now being more or less completely controlled by growing resistant varieties. But a vast amount of laborious work and research is required to insure

sound and substantial progress in breeding resistant varieties. It is not always easy to combine disease resistance with other required characters. Neither is it always easy to combine in one variety resistance to all the important diseases in the region. And even if a new variety is resistant, it may prove very susceptible to hitherto unimportant diseases. Nor does a variety necessarily remain resistant permanently.

The difficulty of combining all desirable characters in one variety and of foreseeing what is likely to happen can be illustrated by experiences in barley breeding. About 25 years ago there came a demand for smooth-awn barley, for reasons that any one who has shocked or threshed barley will understand. Accordingly, crosses were made between the variety Lion, which had little but its smooth awns to recommend it, and Manchuria, a good barley except for its saw-tooth awns. What appeared to be good, smooth-awn hybrids were developed from this cross, but after they had been grown for some time, they proved to be poor yielders. In seeking the reason, it was found that they were very susceptible to the spot-blotch and root-rot disease caused by a fungus with the euphonious name of *Helminthosporium sativum*. Work was then started by plant breeders and plant pathologists at the Minnesota Experiment Station in attempts to produce varieties with smooth awns, stiff straw, good quality, yielding ability and resistance to spot blotch. Within a few years two varieties, Velvet and Glabron, were developed and distributed. Several years later, however, it was found that they were extremely susceptible to loose smut, head blight and to some parasitic races of the barley stripe organism.

Because of the nature of these diseases themselves, it would have been difficult to foresee this development. Loose smut causes abundant infection only when there is moisture during the flowering period of barley. Furthermore, the infection does not become apparent until a year after it has taken place. And at the time when the breeding work was done, no method was known, without absolutely prohibitive labor costs, of artificially inducing an epidemic in order that the relative resistance of hundreds of hybrid lines could be learned. Such a method has been devised within the last few years, however, and will be of great aid in future work. As concerns head-blight, it was not known, even by the most competent pathologists, that it could cause such terrific epidemics in barley; it was considered primarily a disease of wheat. Then, too, epidemics usually develop only when there is warm, moist weather during the earlier development of the barley kernel. And epidemics did not develop during the years when the varieties were being produced. Perhaps the breeders should be criticized for not having furnished the right kind of weather. They do now. By growing hybrid lines to be tested under huge tents, watering frequently to maintain high humidity and spraying the plants frequently with a suspension of the blight spores in water, artificial epidemics are produced so that the relative susceptibility of varieties and hybrid lines can be determined. But this method was a gradual evolution, involving extensive studies by the Wisconsin and Minnesota agricultural experiment stations and the United States Department of Agriculture, of the head-blight pathogen and the factors affecting its development. Even after preliminary, empirical experiments had shown that plants under small muslin cages were more likely to become heavily infected with head-blight than those outside, what would a guardian of purse-strings have said to a request for funds to construct tents under which to breed barley? The realization that weather had to be made to order in an investigation of this kind was a slow evolution,

just as were the necessary principles on which the breeding procedure must be based. And, unfortunately, many of these principles were learned during the breeding work, or even after it had been done; they were needed beforehand. Surely, if we learn at all from past experience, it must be evident that research should precede practice and guide it.

Nevertheless, head-blight has been very destructive in many regions in recent years, as it not only reduces yields greatly but may also make the barley unfit as feed for pigs, because it makes them violently sick. Therefore the development of resistant varieties is urgent. But no varieties of barley now known in this country seem to be sufficiently resistant. Obviously, then, a search must be made in other countries, an important job for plant explorers. And possibly it may be well to remind ourselves that plant explorers who search for plants in many distant lands are not looking only for the curious and bizarre but often for plant varieties that are essential to the solution of just such problems as the barley blight problem. Their work usually is hard and often hazardous, but it is essential. They must find resistant varieties, which often are inferior in other respects. Then the breeder must cross them with otherwise good varieties and attempt to get the desired combination of characters in hybrids.

The complexities of some breeding programs are well illustrated also by experience with wheat. About 1907 the United States Department of Agriculture and the Minnesota Agricultural Experiment Station embarked on a program of developing stem-rust resistant wheats. Bread wheat varieties were susceptible, but many durum or macaroni wheat varieties seemed resistant; therefore crosses were made between durum and common wheats in the hope of combining the bread wheat character with the rust resistance of the durums. Many of the most resistant hybrid lines were so susceptible to root rot that they were discarded, or the root rot automatically eliminated them. It also was found that there was linkage between the durum character and rust resistance; those hybrids that were rust-resistant also had the quality of durum wheats and not of bread wheats. This was discouraging, and some thought that it would be impossible to combine rust resistance with other desired qualities. Advances in knowledge of plant genetics, however, indicated that there might be "crossing over," that there might appear an occasional hybrid in which the bond between durum quality and rust resistance was broken. The obvious procedure was to grow large populations of hybrid lines. This was done, and finally a few plants in one line out of about 1,000, from a cross between Marquis and Iumillo durum, were found which combined the desired characters of rust resistance with bread wheat characters. From one of these plants the variety Marquillo was developed. However, flour made from Marquillo is so likely to be off color that it is no longer recommended and has been replaced by better varieties.

One of these better varieties is Ceres. The first step in its production was the development of the variety Kota, which originated from some resistant plants of bread wheat found in fields of durum. These plants were selected, propagated, tested, and the progeny finally distributed. Kota appeared very resistant to stem rust but soon proved to be so susceptible to orange leaf rust, to loose smut and to stinking smut that it fell into disfavor. In addition, it had very weak straw and was therefore likely to lodge badly. It was then crossed, at the North Dakota Experiment Station, with Marquis, at that time the standard bread wheat of the spring wheat region. One of the hybrid lines was developed into the

variety called Ceres. Ceres has far stiffer straw than Kota, is in general better wheat, and appeared to be equally resistant to stem rust, and certainly no more susceptible to the smuts and leaf rust than the Kota parent. It was moderately to highly resistant to stem rust and withstood a number of rather severe epidemics very well, but it succumbed completely to the terrific epidemic of 1935 because of a combination of factors that were unfavorable to its development and extremely favorable to the development of certain parasitic races of stem rust.

Better rust-resistant varieties than Marquillo, Kota and Ceres are either made or in the making. All three have rusted heavily under some conditions and have other defects. The production of Marquillo had shown the possibility of obtaining resistant bread wheat types from crosses between durums and bread wheats. But this variety is susceptible to root rots, its flour is low in color score, and it is not always so resistant as is desirable. For these reasons one of its sister selections was crossed with a selection from a cross between Marquis and Kanred, a hard red winter wheat, which is immune from a considerable number of parasitic races of stem rust. This double cross, (Marquis × Iumillo) × (Marquis × Kanred), has resulted in the production of the variety Thatcher, which obtained one type of resistance from Iumillo, another from Kanred, and has the spring habit and high quality of Marquis. So far Thatcher has been moderately to highly resistant to stem rust, but there are indications that it may become heavily rusted under some conditions. Furthermore, it is quite susceptible to orange leaf rust. Therefore it and other selections and varieties have been crossed with Hope and H44, two varieties produced by McFadden as a result of crossing Marquis with Jaroslav emmer, which is resistant to stem rust, stinking smut and several other diseases. As in the case of bread wheat-durum crosses, only a few hybrid lines combined bread wheat characters with the resistance of the emmer parent. But Hope and H44 did. Although they are not good wheats, they have been used extensively in recent years as resistant parents in crosses with better wheats. But another complication has arisen. Both Hope and H44 are very much more susceptible to the black chaff disease than the varieties now grown. This, then, introduces another new problem, especially since there appears to be linkage between the resistance to rust and susceptibility to black chaff. That is, rust-resistant segregates from crosses with Hope or H44 as one of the parents are susceptible to black chaff. Possibly this linkage can be broken, or possibly the disease will not be very important. That remains to be seen. Certain it is that many of the hybrids between Hope or H44 and other varieties usually are almost immune from stem rust. But will they remain so under all conditions?

Hope wheat has rusted heavily, not only in experimental tests in the greenhouse, but also under natural conditions in the field, as shown by Abbott in Peru. And why should it not? Disease resistance, like any other plant character, varies more or less with environmental conditions. The practical question is, how much will it vary under the range of conditions in which the variety is likely to be grown? Hope has varied from virtual immunity to virtually complete susceptibility. Seedling plants are highly resistant to many parasitic races but completely susceptible to some. As the plants grow older, however, they are likely to be resistant to all races, because of a combination of characters which make it difficult for the rust to enter and to develop well even if it does succeed in entering. Then why does Hope sometimes rust heavily? The question has been answered, to a con-

siderable extent at least, by investigations made by Helen Hart. Stem rust does not enter Hope as easily as some varieties because the stomata have a tendency to remain closed much of the time. It takes considerable light to make them open. Therefore if there is moisture on the plant long enough to enable the spores to germinate while the stomata are open, the rust enters. The important thing is to have light and moisture for considerable periods of time. Obviously, this combination is not likely to occur often. While light is required for entrance of the rust, it develops well after entrance only under reduced light intensity, its development being sharply checked in full sunlight, just the opposite of the case with most varieties. Clearly, then, Hope will become heavily rusted only when a rather unusual combination of conditions prevails. There must be abundant inoculum of one or more virulent physiologic races, there must be light for considerable periods while the plants are wet to permit entrance of the rust, followed by lowered light intensity to permit the rust to develop. These conditions must be repeated several times in order that an epidemic may develop. Therefore Hope is not likely to become heavily rusted very often, but it has been heavily rusted and no doubt will be again. It is resistant under more conditions than most other resistant varieties and is therefore extremely valuable; but it is not universally resistant, and this fact may as well be recognized now as later.

Even if the mechanism of resistance of varieties remained constant, their disease reaction might vary greatly because of the existence of parasitic races. Investigations during the past twenty years have shown that there are numerous parasitic races of the cereal rust fungi, the cereal smut fungi, those causing root rots of cereals, and a host of others. In fact, it appears now that most species of parasitic fungi comprise races that may be alike in appearance but quite different in their parasitism. Approximately 150 such races of the wheat stem rust fungus are known. Consequently, some varieties are resistant in some years and in some localities and completely susceptible in others. The variety Kanred was distributed a number of years ago as a rust-immune hard red winter wheat. It was soon found, however, that it was completely susceptible to some parasitic races and that it may rust heavily when and where these races are present. The same is true of the durum wheats. Until about 1923 they were considered highly resistant to stem rust. However, in that year an epidemic developed on them, and the same thing has happened in a number of subsequent years. Whether most durums rust, then, depends on the particular parasitic races present. And to complicate the matter still further, there is some evidence that a variety may be resistant to certain races at certain temperatures and susceptible at others. In other words, environmental factors determine whether certain races can attack a certain variety or not. This whole series of complications, together with genetic and pathological studies on the nature of rust resistance, led to the breeding of varieties with "adult" resistance. This simply means that older plants of some varieties, because of structural or physiologic peculiarities, are generally resistant to all parasitic races under natural conditions in the field, even though the seedlings may be susceptible. This, then, seemed to be the answer to the challenge of parasitic races. Unfortunately, however, "adult resistance" may vary, as already mentioned in connection with Hope wheat.

What has been said about stem rust is equally true of other diseases. The fact that there are so many parasitic races and that the resistance of varieties, even to single races, may vary merely

shows the complexity of one of the most important problems in plant protection, the breeding of resistant varieties. The difficulties have not been magnified. Numerous examples could be given in support of this statement. Many varieties of wheat were resistant to stinking smut, only to become susceptible; some wilt-resistant varieties of flax have become susceptible; some smut-resistant varieties of sorghum have lost their resistance; certain mosaic resistant varieties of sugar-cane are no longer resistant. Nature is not static; it is dynamic. The plant disease problem is not static; it changes. And why should it not change? The pathogens that cause disease change.

Extensive researches during recent years show conclusively that new parasitic races of pathogenic fungi arise through mutation, hybridization and probably through chance assortment of nuclei. That new races arise through mutation is perfectly clear from studies on certain smut fungi; that they apparently arise in several fungi through chance assortment of nuclei seems likely from recent studies in California and elsewhere; that they arise commonly through hybridization in some of the ascomycetes and in the rusts and smuts is proved beyond question. Hybrids have been made between biotypes within a species, between different species and even between different genera. In fact, the union of lines of different sex is prerequisite to infection in the rusts and smuts. While we are breeding disease-resistant varieties of crop plants, nature is breeding new races of crop pathogens. Man probably can keep ahead of nature, but to do so he had better know what nature is doing to checkmate him. To proceed blindly in the dark is not likely to lead to greatest progress. Research must show the way.

It must be apparent, even from the few examples given, that obstacles to the development of resistant varieties are not necessarily insuperable; in many cases they are not, because they already have been overcome. In other cases they are still to be overcome. It is not desired to magnify difficulties and cast a pall of pessimism over breeding as a method of protecting plants. Quite the contrary. The fact is that breeding is the only hope of controlling some of the most destructive diseases. The need for resistant varieties is acute. What it is desired to emphasize is the urgent necessity for an appreciation of the complexities inherent in many breeding problems, for fuller understanding of the needs in solving them, and for wider vision with respect to the scope of necessary researches and their results. In some cases suitable resistant varieties already are available, but in many others there are no commercially desirable ones. Plant exploration may be required; testing and sorting is necessary. The genetics of the crop plants must be studied; the number and parasitic capabilities of parasitic races of the pathogen must be learned; studies should be made to ascertain whether new races are arising; the nature and variation of resistance should be studied; and ecologic studies must be made of the host plants in relation to the disease. Only on the basis of such studies can serious mistakes and disappointments be avoided and permanent progress be made. For it is important to know not only what has happened but also what is likely to happen under certain conditions. Even so, it may be necessary to replace varieties periodically, as commercial requirements and natural situations change.

There has been great progress in plant protection, and the prospective accomplishments are still greater. Emancipation from empiricism will be one of the most significant. This will require more intimate knowledge of crop plants, elucidation of the nature of disease inciters, such as viruses, more detailed life his-

tory studies, finer analysis of the rôle of environmental factors in the development of disease, epidemiology studies to improve predictability of disease outbreaks, vast improvement in specific control measures and more precise knowledge regarding their application. Botanists of many persuasions must cooperate in these studies even more closely and sincerely than in the past.

"The price of a sound, comprehensive national life is in these times widespread and intelligent scientific research." This quotation from Angell is applicable to plant protection, as well as to problems in general. Botanical science can promise man better varieties of crop plants and can show how better to protect them against diseases and other hazards. But to accomplish this there must be provision for basic research, to discover facts and formulate principles; experimentation, to determine when, where and how they can be applied profitably; and education, to incorporate them into practice and capitalize on their value. We need not only fuller knowledge, improved skills, and better techniques, but also a deeper and more widely diffused sense of obligation to science and to society and a determination to discharge it equally faithfully and honestly to both. This statement is platitudinous, of course. But, like many other platitudes, it often is ignored and can be emphasized with profit. And it is not mere parroting of a newly popularized slogan. Many investigators and most teachers were thoroughly imbued with the idea long before attention was focussed on it in recent years. What is lacking in many cases is not the spirit but the substance to enable science to serve society.

There must be much good research, but much of it must be good for something. Only when there is broader realization of the ultimate value of basic research, not only to clarify situations, but especially to provide a reservoir of facts and principles for future emergencies, will it be possible to proceed as intelligently and effectively as necessary in plant protection. If past experience teaches anything, it teaches that the most fundamental research often is the most practical in the end. Plant disease situations continually change, because crops and pathogens and conditions change. New problems continually arise. Only by elucidating principles and accumulating wisdom through research can we foresee possible future developments and prepare to meet them. Apathy and lack of comprehension, rather than antagonism, are the greatest obstacles to research and progress. Many people still have a child-like faith that science can perform miracles. A new disease or insect pest menaces an important crop. The formula is to provide money and demand a miracle. "Miracles of science" may be a good figure of speech, but most scientific miracles are the result of long and laborious search and research, repeated many times. We hear much about preparedness. Preparedness is essential in plant protection, but we had better prepare for the future before it arrives instead of after it is present or past. And preparedness must be based on research.

THE COMMERCIAL-RESIDENTIAL ECOTONE

AS EXEMPLIFIED BY HASTINGS, NEBRASKA

By Dr. THOMAS F. BARTON

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GEOGRAPHERS have adequately discussed as units many rapidly growing towns and cities. In an increasing number of articles writers have also delimited portions of urban areas devoted to a definite economic function. These functional divisions, such as the commercial, residential and transportational, have been treated as to importance and interdependence. Up to the present, however, almost no attention has been given to the description and interpretation of the transitional zones between the functional divisions.

Although the transitional zones between functional divisions are not large, they are often a conspicuous part of the urban landscape. An attempted horizontal expansion of one functional division means a conflict with the use already established in the invaded area. The function giving the greatest economic returns usually encroaches upon and replaces a less productive one. Within the zone of conflict, a landscape is produced which represents neither of the two divisions, but, rather, represents a result of a combination of their characteristics.

These transitional urban zones or areas vary in size, being large and very conspicuous, usually in cities of over 50,000 inhabitants, and smaller and less well developed in cities of 10,000 to 25,000 inhabitants. In some towns, only a few features of a changing zone are evident. However, where the transitional zones are of sufficient size to be delimited because of their distinct landscape character, they become a subject for discussion.

ENLARGEMENT OF HASTINGS' COMMERCIAL CORE

In a reconnaissance survey of Hastings, one's attention is called to the fact that the commercial division of the city is expanding horizontally toward the north and west. The direction of growth is indicated by the new commercial structures, the residential structures utilized by retail or service businesses, and the isolated houses which stand surrounded by commercial establishments. The extent of the horizontal expansion becomes conspicuous when one contrasts it with the slight vertical expansion, as shown by the low profile of the commercial core. For example, the business district contains only two buildings of more than five stories; the others are buildings of two stories or less. Moreover, the vigor of the commercial encroachment upon the residential division stands out in bold relief when contrasted with the unoccupied lots near the heart of the commercial division.

These idle lots are found here for several reasons. They are in most cases held by the railroads or citizens for speculative purposes. Also, taxes are levied on property and if a lot is vacant the tax is low. Some of the lots formerly contained buildings, but these were torn down and new buildings have not as yet replaced the old ones.

Therefore, in spite of slight vertical expansion and vacant land near the heart of the city, some establishments now occupy and others are moving into structures formerly used as private residences. The residential function is forced out and replaced by a commercial

one. This change brings a transformation to the physiognomy and the structure of the individual building and the entire block. Thus there is a tension zone or an area of "no man's land" between these two economic divisions, the commercial function steadily winning as it encroaches upon and absorbs the adjacent residential area until the movement is blocked by some cultural or physical feature. Since geographers have not coined a term for this land utilization area with its distinct landscape, the writer suggests the term "commercial-residential ecotone."

Origin and Use of the Word "Ecotone"

"Ecotone" is a Greek word meaning tension-zone or a battle front along which two forces are endeavoring to occupy the same space, but with the final result that one wins the struggle and advances over the conquered territory.

Plant ecologists use this word to designate the area in which a struggle is taking place between two plant formations as one tries to retain an areal space while the other fights to gain new ground by encroaching upon its occupant. For example, the buck brush from an erosional ravine tends to spread into the prairie occupying the interfluvial area. This plant sends out rhizomes, which reach several feet beyond its border into the prairie's area. The new buck brush plants in turn send out roots, and by continuance of this process the brush forces the edge of the grass to recede. The area which is being contested is a transitional zone occupied by both forces for a period of time until the advancing force overcomes and eliminates the defending one. However, once the encroaching force completely occupies the area, the former contested ground becomes the site of the invading plant and is no longer a transitional zone or ecotone. The ecotone is only the zone of competition.

In the economic battle, herein set forth, the contestants are, on the one hand, the commercial and, on the other, the residential divisions. For this reason, the term commercial-residential ecotone is used.

The Hastings Commercial-Residential Ecotone

Description and Interpretation: The commercial-residential ecotone of Hastings occupies an area which forms a pattern resembling roughly an imperfect ziggarut with its highest point at the intersection of St. Joseph Avenue and Sixth Street and its base resting on the alley between Third and Second streets (see Fig. 1). This block-like form is due to the rectangular arrangement of the streets and lots prevalent in most American cities, but which are not typical of most European ones. The serrated boundary line is representative of an ecotone, since the encroachment of business establishments upon the residential area results in changing the functions of lots, but not of complete blocks.

Theoretically, the horizontal growth of a commercial division over a level terrain without cultural or physical barriers should be radial. If a line were drawn connecting the tip of each step in the ziggarut-like pattern, an imperfect semi-circle would become evident. The radial pattern on the north is produced by an expansion which has not been interfered with by physical or cultural features.

The ecotone does not completely surround the commercial division of Hastings. Moreover, such a phenomenon may never develop on the southern edge of the business district. Nearly all the light retail and service establishments are located north of the Chicago, Burlington and Quincy railroad and west of the St. Joseph and Grand Island lines. Consequently, one would expect any new business of this type to locate in juxtaposi-

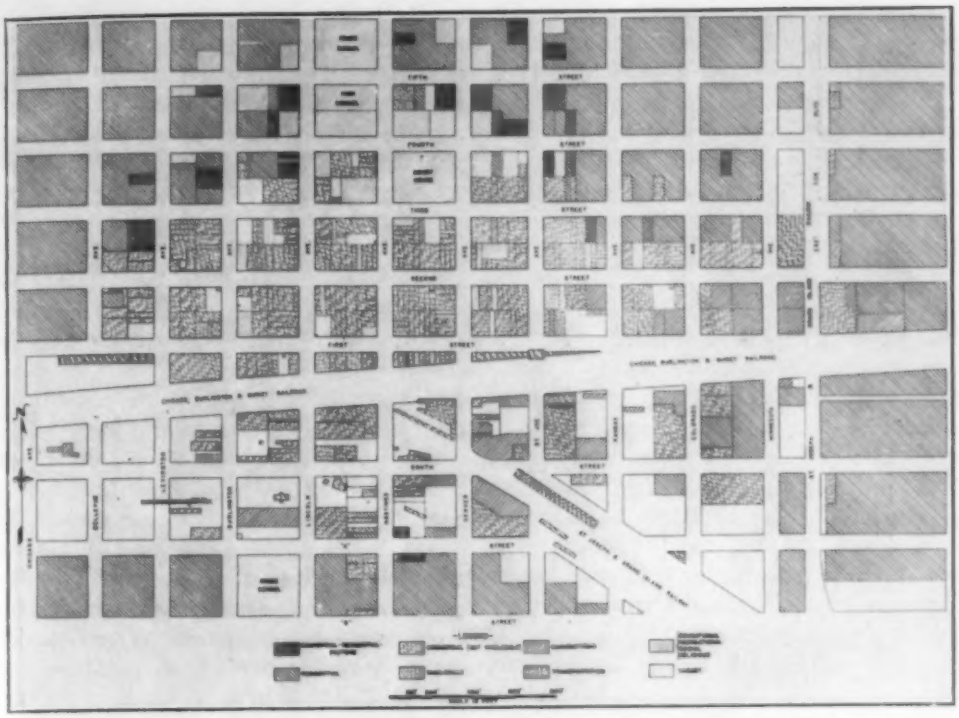


FIG. 1. THE BUSINESS DISTRICT, HASTINGS, NEBRASKA

tion to the old. Approximately 75 per cent. of the urban population is on the north and west side of these tracks; thus, the greatest part of the urban markets are also found here. The commercial area south of the tracks is not esthetic in appearance, because of junk yards and farm produce stores. Light retail and service businesses generally avoid such an area.

Moreover, the heavy retail and service establishments, found in the southern part of the commercial core, are not the type that makes use of residential structures. They consist in the main of garages, filling stations, lumber yards, implement houses and farm produce stores.

The ecotone area contains a number of residences which have been converted into apartment and rooming houses. This conversion will likely continue on the inner fringe of the residential area surrounding the northern part of the commercial division because: (1) the residential area north of the commercial division excels that to the south, (2) the greatest employment is found north of the Chicago, Burlington and Quincy tracks and therefore the residents to the north would be nearer to the place of work, and (3) the railroad tracks and the long freight trains act as barriers.

Although the numerous cultural features listed here prevented an ecotone from developing south of the business district, the decaying residential border adjacent to the northern side of the commercial division enables an ecotone to develop. This area contains old houses, many of which were constructed during the first decades of the town's growth (1872–1892). A large number of the residences are of old architectural design

and lack modern conveniences. In many cases exterior and interior remodeling would exceed the value of the building.

Furthermore, this residential periphery adjoining the retail area on the north has become less desirable because of the change brought about by the business atmosphere in the last two decades. With the coming of paved streets and the extensive use of trucks and automobiles, noises, dirt, odors, heavy traffic, fumes and gases have vastly increased (see Fig. 2).

Several economic forces are instrumental in stimulating the migration of retail and service establishments. Marginal businesses have been influenced by some profitable competitors to migrate farther and farther from the commercial core in order to cut cost of operation. These migratory establishments evade the high rents[1] demanded in the core of the business district. They usually are marginal producers or belong to a class of business which does not yield the greatest net returns, and therefore can not afford to locate in choice sites. As a rule, commercial land utilization gives greater net returns than residential utilization; therefore, the residential use gives way with little resistance before the increasing pressure of encroaching businesses.

The peripheral retail and service establishments in Hastings are laundries, small grocery stores, repair shops, tailor shops, paint, print and radio shops, doctor's offices, beauty parlors, rooming houses and remodeled residences with apartments. These businesses are easily adjusted to old residential structures. Shingle signs on porches, billboards in yards or large paint signs covering the front windows indicate that a former residential structure is now performing a commercial function (see Fig. 3).

A city block transformed in function is also transformed in structural appearance. In the process of change, the wooden structures are replaced by brick buildings. Trees and lawns disappear. Since the price of land is high, all ground must be utilized, consequently detached structures are replaced by buildings closely constructed and forming a solid

[1] The writer is unable to say whether rents were excessively high in the business district or not but was told by the people in charge of businesses located in houses that rent was too high for them to occupy commercial buildings in the business core (interviews, summer, 1933).

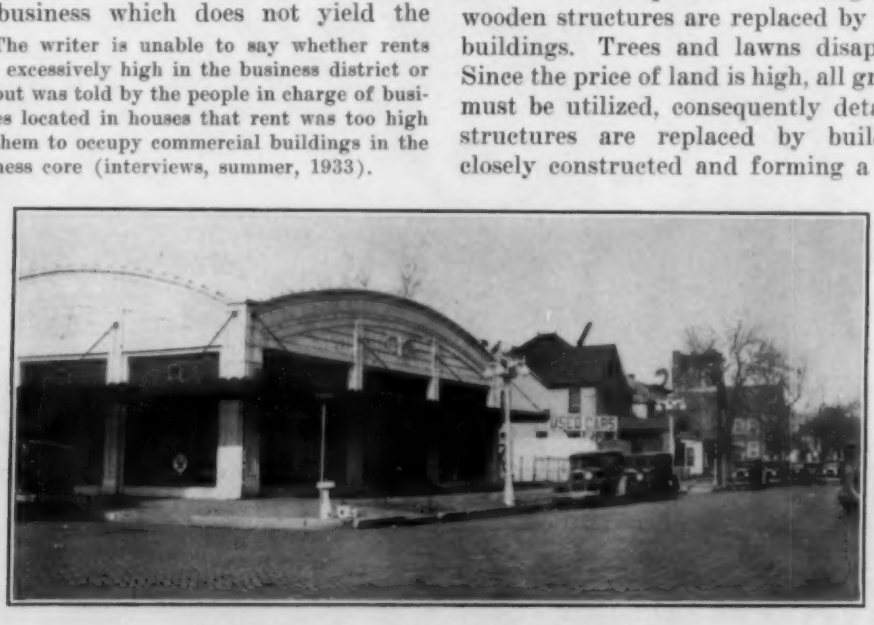


FIG. 2. A NEW COMMERCIAL DISTRICT

A GARAGE AND A USED CAR YARD HAVE MADE THIS BLOCK UNDESIRABLE FOR PRIVATE RESIDENCES. HOUSE NO. 1 HAS BEEN CONVERTED INTO A TOURISTS' INN; NO. 2 IS DILAPIDATED AND EMPTY; NO. 3 IS A BEAUTY PARLOR.

FIG. 3. BUILDING SERVING A DUAL FUNCTION

THE FIRST HOUSE WITHIN THE ECOTONE AREA SERVES AS A HOME AND BEAUTY PARLOR; THE ADJACENT HOUSE IS A RESIDENCE; THE THIRD HOUSE CONTAINS A CLEANING ESTABLISHMENT.

row along treeless, lawnless and often auto-crowded streets.

Manner of Encroachment: The encroachment of light retail and service establishments with advancements made along certain streets, or lots in one block, takes place along a serrated front. The advancing commercial uses invade a certain district, and, offering a greater net income, either replace the residential function, or the commercial and residential one may share the same structure for a period of time. The buildings first invaded are usually of least residential value, are in a strategic business location, or are occupied by persons engaged in a certain type of business. Since some residences in a block are of more value than others, and their owners wish to use them a few more years as homes, the migratory enterprises do not present an even advance.

Once commercial establishments enter a few buildings in a block they act as a nuisance and become wedges for further commercial development. Consequently, as the number of structures devoted to commercial purposes increases, the remaining residences depreciate in value. Moreover, as the desirability of a location for residential purposes decreases the taxes become proportionally higher and residential value decreases; therefore, the owners are forced to liquidate and sell their property or change its use.

The residential utilization of land is not an intensive one when considered from the standpoint of net income. It is apparent that a lot in the residential area in juxtaposition to the retail one has its value increased as the commercial core expands horizontally in its direction. The failure of the residential function to resist the commercial one is due primarily to the residential use being the lowest income-producing utilization of urban land.[2] As a city's business district moves in any given direction, the value of blocks and lots for commercial purposes in its path is greatly enhanced. Thus, in time, the residential function yields to a more remunerative use.

In the ecotone, as a matter of economy, the residential structures are often used for commercial purposes. The cost of supercession from a poor function to one bringing a larger net income is high. Necessary factors to be considered here are: "The direct and indirect cost . . . of service units in the present improvement which must be sacrificed for the new improvement, the expense of removing the present improvement, and the loss of income suffered from the time

[2] H. B. Doran and A. G. Hinman, "Urban Land Economics," p. 522.

when the present improvement is vacated until the new improvement is in full use."[3]

Establishments found in the ecotone do not have an income sufficient to make possible the destruction of old structures and the erection of new ones. In Hastings, these establishments are marginal economic producers. High rents made them unprofitable, and merchants were forced to move from the center of the business district to this transition zone. Some of the enterprises, however, developed in the area, and came into being because of the economic opportunities offered. Therefore, the profits of these migratory establishments do not warrant the destruction of the present structures.

In the early stages of encroachment many of the residential buildings serve a dual residential and retail function. In this manner, the cost of supercession is reduced to a minimum. Thus, a saving is made by extending the physical life of these structures beyond their functional use rather than immediately razing the structure because economically a change in function is desirable. The process of change is a gradual one, without abruptness and not susceptible to large financial failures. As the establishments prove successful the increase in volume of trade necessitates the use of the entire building. Once a business is thoroughly established on a paying basis, capital is accumulated and the old residence is replaced by a new commercial building.

Existence of Commercial-Residential Ecotone

The commercial-residential ecotone is not a peculiarity found only in Hastings, Nebraska. Similar transitional zones with their characteristic features may be found in many growing cities. This phenomenon is believed to be typical of growing cities where the area occupied by commercial divisions has been surrounded and limited by residential ones of an earlier period in the city's history. In some cases, the city may have developed to the extent that the surface area is no longer adequate to accommodate the increasing commercial establishments responding to the city's growth in trade.

In most cases, the commercial core expands gradually and the migratory establishments invade the residential area as the city's volume of business increases. Expansion must be either vertical or horizontal. Unless it is checked by salient physical features, public buildings or an ordinance zoning law, expansion is usually horizontal until distance from the center of the commercial core hinders further growth.

Encroachment of this type is conspicuous and has been observed by the writer in Peoria, Illinois; Memphis, Tennessee; Kenosha, Wisconsin; Lincoln, Nebraska; and other cities, but their ecotone areas have not been studied in detail as in Hastings, Nebraska.[4]

Because transitional zones do exist between the commercial and residential divisions of some cities and because these zones are large enough to be delimited and have a distinct landscape, the writer believes that these areas are worthy of geographic treatment. Since a name for these transitional areas does not exist, the term "commercial-residential ecotone" is suggested.

[3] *Ibid.*, p. 213.

[4] Hastings, Nebraska, was mapped in detail by the writer during the summer and fall of 1933.

THE HUMAN SIDE OF SNOW

THE SAGA OF MOUNT ROSE OBSERVATORY

By Dr. J. E. CHURCH

METEOROLOGIST, NEVADA AGRICULTURAL EXPERIMENT STATION; DIRECTOR, MOUNT ROSE WEATHER OBSERVATORY

LAST year the reminiscing columns of the press recalled the fact that 40 years ago two men from the University of Nevada climbed the bare peak of Mount Rose, 10,800 feet, overlooking Lake Tahoe, on New Year's Eve. The same news columns have now announced that this past September the leader of that party has organized the International Commission of Snow at Edinburgh. Thus in the space of less than half a century a trip made solely in love of winter nature has given birth to snow surveying and to a world organization whose central purpose is to study the scientific and human aspects of snow and ice, now designated as a new field in hydrology and by some as a new science with the forbidding name of eryology (the science of cold).

Snow surveying may have originated in Europe, but no trace remains. It started anew in Maine in 1900, under the leadership of Charles W. Mixer, to know the prospects for filling the storage lakes of the upper part of the Androscoggin Basin from the snow cover for a year-round regulation of flow and to give a warning of the spring runoff or flood on the lower part of the river. This pioneer effort still survives. Mixer's object was to determine the water content of the snow, which in terms of depth only is an elastic substance incapable of accurate measurement.

Snow surveying started anew in New York in the experiments of Robert E. Horton to create a weighing snow-sampler in the study of the physics of snow.

But somehow the originality of the West and its need of water caused the Mount Rose method of snow surveying to spring up independently and persist against inertia and opposition until it has covered the West and returned to the East, where it now covers the Adiron-

ERECTION OF MOUNT ROSE OBSERVATORY, 10,800 FEET. ARRIVAL OF FIRST PACK TRAIN AT SUMMIT.

OUR SLED AND BASSWOOD WALKING STICK.
SLOW UP, FAST DOWN.

dack Region of New York, the Connecticut Basin of New England, portions of Canada and west central Newfoundland. But its route has also been westward to far Australia, where the state of Victoria is generating power from snow. India and Argentina are completing the world's circuit, while Greenland and the Alps have become laboratories in glacier study.

The story has earlier been intimated in the *Scientific American* under the title, "The Snow-Surveyor of the Sierras," January, 1933, by the author of "Eiley Orrum," but its details have become a saga of mountaineering, which the writer and leader of that midwinter party desires here to relate.

All movements grow slowly. The whole undertaking started unexpectedly in the Sierra Nevada in December, 1895, in defiance of a sneer at my proposal to make a belated ascent of Mount Rose after the snows of winter had fallen.

MAN-TALL REFUGE CAMP ON MOUNT ROSE, 9,000 FEET.
SLEEPING BAG TREE AND ORIGINAL REFUGE HUT IN RIGHT FOREGROUND.

ORIGINAL REFUGE HUT, 9,000 FEET. CRAWLING OUT.

We had nothing but rubbers and one pair of webs for two, and the mountain rose a mile above its base. The route then laid out has remained the usual route to the present day.

After returning from Germany in 1901, where winter resorts were maintained on the lower peaks, Mrs. Church and I ascended the Sierra to elevation 9,000 feet, where we lived for a week at Christmas in fragments of cabins without heat, except for cooking, and roamed in a world so far removed from our own that only the soft whistle of the distant locomotive penetrated the stillness.

A fifteen-hour ascent of Mount Rose, at the apex of our range, was made with only the starlight to guide us back along the cornices of snow. Each evening we lighted a tiny star from some dead pine to assure the anxious watchers in Reno that we still were there.

The adventure spirit was now taking fast hold. I had had belated dreams of

PRECIPITATION TANK ENROUTE TO SUMMIT.

FIRST INSTRUMENT SHELTER ON MOUNT ROSE.

the Arctic, but realized that now I must find my enthusiasms in my own back yard. Only one man in California had so far ventured into the winter wild. He was Bolton Coit Brown, of Stanford. At that time I was ignorant of the great adventure of John Muir up Mount Shasta and decided to dynamite the Californians into setting foot on snow for the sheer joy of it.

So in 1906, in midterm time when the snow was deepest, I persuaded G. F. Marsh, of Lone Pine, to guide me up the east face of Mount Whitney (altitude 14,501 feet) to make the highest winter ascent possible in the United States. We had a week of vision over the "Land of Little Rain" and blue Death Valley from out the skirts of the storm playing over the crests of the High Sierra. We mounted to 13,300 feet, where the monument on Mount Whitney could be seen just overhead, but stopped when ominous cracks opened in the snow above us and rocks dislodged by our feet fell with a sickening echo from the cliff on which we stood.

Three utterances were spontaneously

TAKING TINY INSTRUMENT SHELTER TO SUMMIT OF MOUNT ROSE.
"BILLY'S" THIRTEENTH FALL, THE AUTHOR AT BILLY'S HEAD.

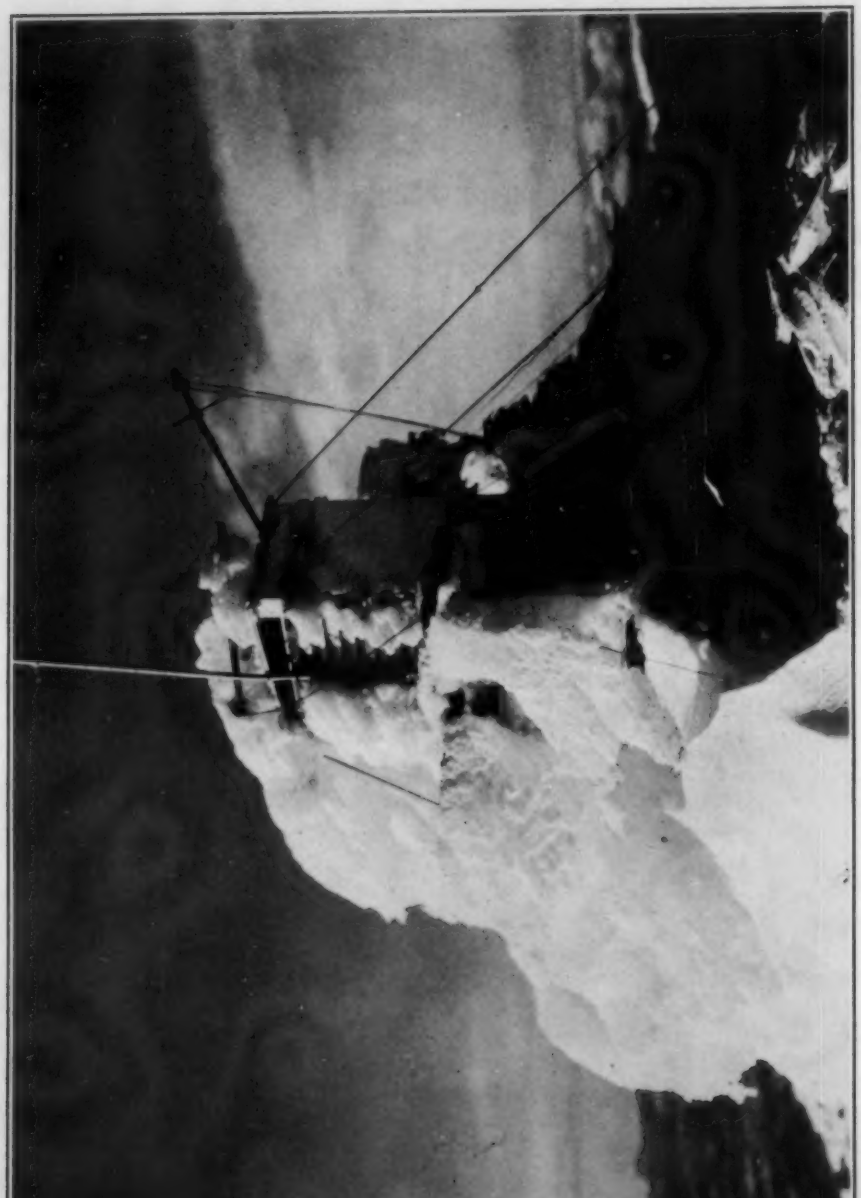
born on this trip: "Tell them I died happy," a gay retort to a worried colleague, who asked for some last message if I failed to return; "Church, take no chances," which echoed in my ears from my trusting university president, who let me go despite public opinion; and "The call of the wild is strong but the home call is stronger," forced reluctantly from me when on our belated return I saw a tiny figure in white, the wife of my guide, with a baby of six weeks, gazing from the gate into the night at the silent hills into which we had vanished.

Such was the background against which Mount Rose Observatory was born. I had gone to the hills for pictures and pleasure, but to the public I was merely a great fool. So the humanist decided to become a scientist and a "hero," yet still take his pictures on the side. Now every one goes to the winter hills—some even in oxfords—and heroism has vanished. When my scientific work is rounded out, I hope to return to my pictures with the public's full approval. Thus tradition slowly changes and new view-points and progress are attained.

MOUNT ROSE OBSERVATORY IN ITS FIRST WINTER.

Two great experiences stand out in this

A WINTRY DAY ON MOUNT ROSE.



WRECKED BY ICE FEATHERS.

SUMMIT OF MOUNT ROSE FULLY EQUIPPED.

quarter century of toil and fullness of life. First, the abundance of knowledge that nature will pour into your lap if you will come within her reach. Second, the zest of adventure. In the first, we had no training other than that required of an Arctic explorer, *viz.*, the willingness to endure and the desire to observe. Our special training had been in the classics wholly. In the second, a self-selected group of adventurers (many dropped promptly out) became devotees to the adventure and by their man power put over the task that would have crumpled one man working alone. However, this lure almost brought destruction to the work, for when the federal inspector once accompanied the party to Tahoe, in which for his sake the trip was confined to cruising and pointing out the snow courses from far below, he too caught the germ as he handled the boat's tiller, but considered the project merely a summer outing at government expense.

At the time of the trip to Mount Whitney, winter temperatures at high elevations in the Sierra were wholly unknown, except for a minimum reading on Mount Ritter, I believe, from a thermometer cached over the winter in the rocks. On my return, I offered to take maximum and minimum temperatures on the summit of Mount Rose every month for a year if instruments were furnished. They came promptly. A tiny instrument shelter was taken on a saddle pony over rotten snow to the summit.

A minimum reading of zero in August —false but at that time not explainable —started the Associated Press to comment and the scientific world to take interest. A thermograph with time range of ten days was installed. Every two weeks, irrespective of the weather, a party made the ascent to bring down the record. Sleeping bags were hung from a pulley on the limb of a giant alpine white pine at 9,000 feet, and here the members spent the night and split— one going alone up the wild summit, while the other remained in reserve, with instructions to follow in case the other had not returned in four hours.

We found nature a genial though austere friend if we heeded her moods, but relentless if we were heedless. Thrice in one week we struggled to gain the record. The first party spent the night in an open grave (for it so looked) dug in the snow to avoid a gale of 70 miles at zero temperature. But though they fought their way to the summit,

they were powerless to remove the tiny record sheet from the instrument and were forced to hold their hands over their faces to keep the latter from freezing. Then I started alone to retrieve the "disaster," threading my way in dense fog along the ridges and sounding the canyons by my voice. But the gale sprang up a second time, carrying the soft snow into the air in density and blindingness like flour in an overturned flour mill. So intense was the gale that my basswood walking stick, whose ends were held firmly by the snow and my clenched hand, vibrated like an aeolian harp. Suddenly a crack opened in the clouds through which I discerned distant hills and my whereabouts. I had wandered into the wildest part of the mountain. To quiet jaded nerves, I rode a sled at full speed down a thousand feet and mounted my pony for the 15-mile trip home. As I did so, the mountain cleared invitingly. The forecast I had followed had been twenty-four hours too early. Two days later the original party made the ascent in comfort, even leaving their coats behind.

Unexpectedly, the Adams Act for agricultural research was passed by Congress and $500 was available if it could be used within three months. Out of this came Mount Rose Observatory, carried up the mountain on pack horses over the untrailed rocks. So valuable did every stick become that silver dollars laid edge to edge over the surface of the building could not have compensated for the physical effort if built by hirelings. It had been a labor of enthusiasm without

OUR ROWBOAT AND SNOW SURVEY PORTABLE CAMP ON TAHOE.
TAHOE NEVER FREEZES.

CABIN CRUISER, "MOUNT ROSE," IN HARBOR.

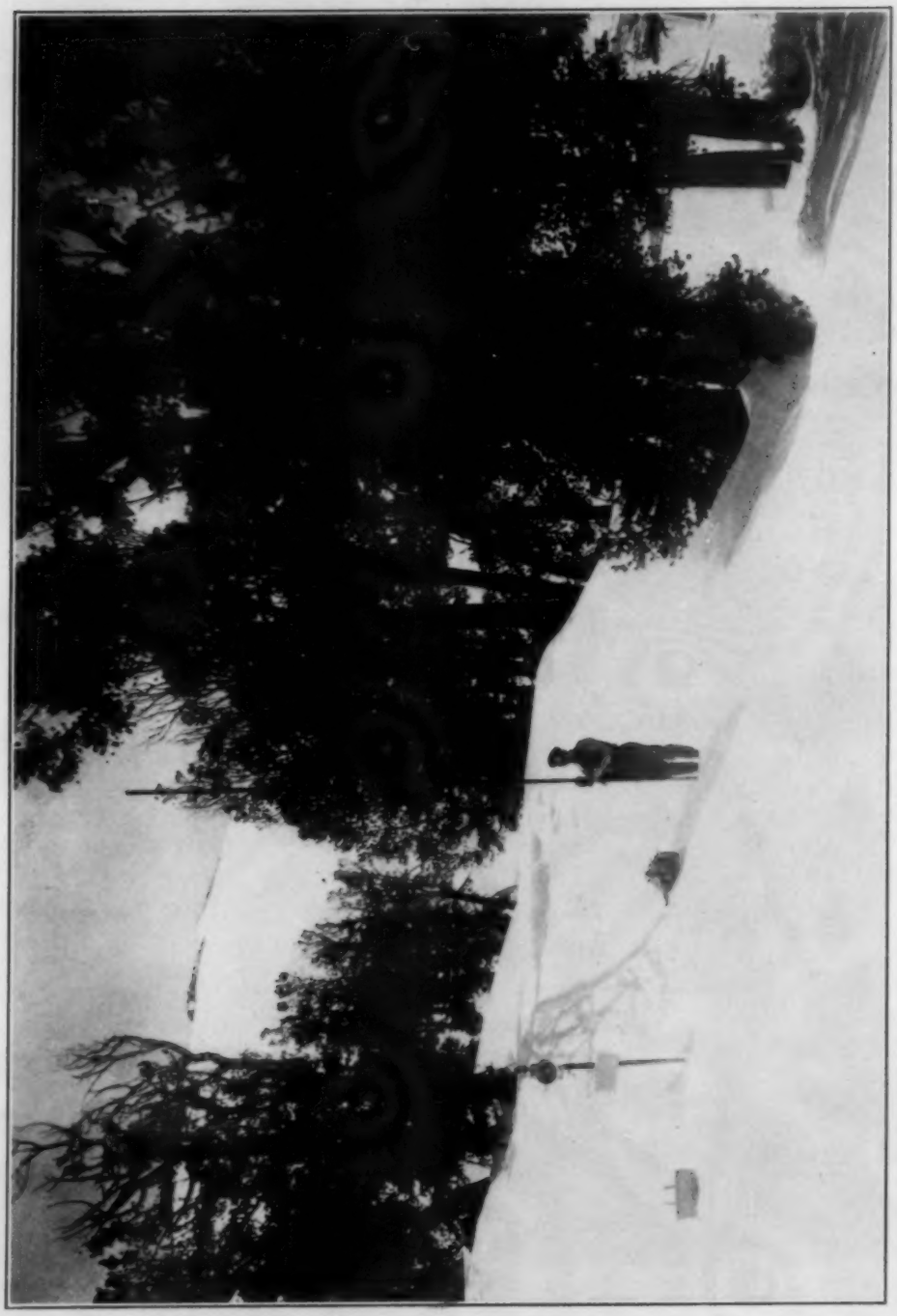
money and without price, with the motto dormant in our thoughts: " 'Fools rush in where angels fear to tread' and are generally successful."

Here again nature was exacting but merciful and lavish of her grandeur. Windvanes, masts, door, electric cables had to be rebuilt to avoid the effect of frost feathers, or lightning that would take bites out of the anemometer cups or steal into the building to set it on fire.

So wild at times was the gale that it would lift me up but promptly allow me to settle gently to the rocks like a stone in a stream, or create such a vacuum above the observatory that the roof would begin to lift from the released pressure within, while the barometer pumped wildly in response. At such times the descent of the mountain became a pleasant swim by lying forward upon the wind and pushing with our feet.

The Refuge Hut was next built at the site of the sleeping bag camp below. At first, a low sandbag hut three men wide and half-a-man tall. A cooking lamp and an immense rabbit skin blanket constituted the furniture. If the dog accompanied the party, he must be the pillow. So close were the quarters that a wood rat once had to fight his way through the tangled hair of my companion to escape from his hole. Here we could lie in comfort (once for 36 hours) while a storm was wearing itself out.

To feel and act like a human, a man-tall hut was next erected, again of sandbags and tar and canvas. A facing of rocks on the windward side protected it from erosion by snow and sand. This hut became the center of our snow surveying, to which we turned in our later study of the effect of mountains and forests on the conservation of snow. Here in comfort we outlasted gales that would have overwhelmed human life if unprotected. One of these left a fin of snow impinged on each tree trunk. In one gale, one of the boys rose up in bed to unhook his trousers from a rafter, remarking when quizzed, "I want my pants if this roof blows off."

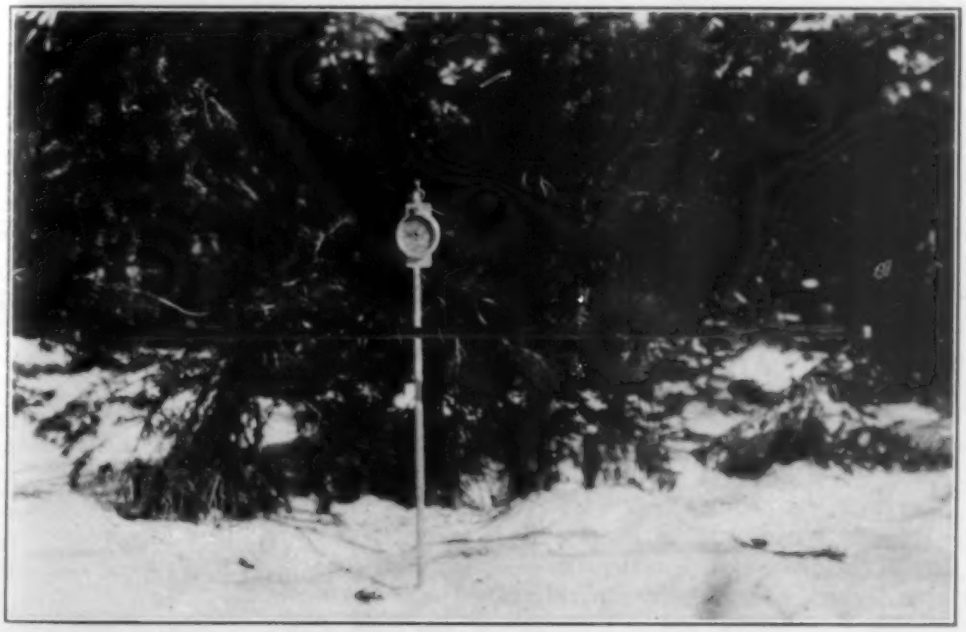


SNOW SAMPLING AT TIMBERLINE ON MOUNT ROSE. THE SNOW IS AS DEEP AS THE SAMPLER IS HIGH.

Enthusiasm begets enthusiasm. The foreseeing college president was urging us on. Our studies of timber and snow, for which the Mount Rose snow sampler had been developed and snow surveys inaugurated, had now been extended to Tahoe, where a never-freezing lake surface and shore line of 72 miles gave access to various types of mountain slopes and forests. Here with rowboat and tent we developed physique, exercising our arms at the oars one day and our legs on the snow the next. The basin became far more accessible than in summer and far more beautiful. Pyramid Peak, Tallac and Freel led us into the depths and heights of the range. The rowboat became the lifeboat tender of the frail motorboat *Skidoo*, which had originally been a boy's pet. Then for fear the motor would fall through the bottom in midlake, the cabin cruiser *Mount Rose* was built to anchor on any shore and ride most gales, however rough.

DRIVING THE SAMPLER IN DEEP SNOW.

The seasons were heavier in those days than in these. Tahoe had risen in 1906–07 a foot and a half above its outlet



WEIGHING A SNOW CORE NEARLY 20 FEET LONG TO DETERMINE WATER CONTENT.

dam. Natives talked of the flood. The heavy year of 1910–11 came with its menace and fear. The Sierra Pacific Power Company begged the use of our snow data to determine how much moisture was latent on the watershed. Thus was born the forecasting of streamflow.

A lake is the least likely spot to begin such a project, for it fluctuates too much to reveal underlying principles readily. But nature was again kind and the relatively heavy winter precipitation of snow in the Sierra as compared with the summer precipitation of rain gave us a preponderant factor with which to work. Luckily we rejected the quantitative measurements usually applied to rainfall and reduced both the snow cover and lake rise to percentage of normal. Then it was quickly seen that the winter snow in percentage of its normal equaled that of the summer rise of the lake. Rivers followed the same rule but more closely. Furthermore, the snow cover was seen to be approximately uniform for long distances along the range.

But divergence crept in—in 1915–16 a divergence of 50 per cent. at Tahoe—and the irate impulsive Southern project manager damned the experiment as a failure. But Major Norboe, chief assistant engineer of California, who was looking for a method while our method was looking for a patron, calmly remarked, "Nature must have butted in." Three years or four of "watchful waiting" and praying that nature would repeat the trick revealed that nature's withholding her spring rain would reduce our expected runoff by 50 per cent. of normal in lakes and 25 per cent. in streams.

The work inevitably spread. The Sierra became a gigantic laboratory filled with alluring vistas of broad principles and individual though minor problems. The Colorado, Columbia and Bow River systems were investigated to determine the feasibility of snow surveying and streamflow forecasting there, that no problem might escape. Even precipitation and runoff on the Continental Divide were drawn into the vortex of discussion. The playful charge was made that we were trying to annex the world. Such is the penalty of a lure.

But snow surveying became a football game of opposing forces, one force being its own seeming vitality and emergency patrons, the other force seeking to close out an impracticable venture. The continuity of its life was due to Major Norboe, of California, and Governor Emmet D. Boyle, of Nevada, and the water users of the latter state. Times have changed. The forecasts of runoff are given a credence that brings fear of error rather than satisfaction to the forecaster. Economic life or death has at times hung in the balance. Adventure is being taken out of snow surveying. The courses are now measured at the base of the highest peaks, and some snow survey systems involve 100 miles of mushing. Cabins, some with Santa Claus chimney entrances where the snow cover lies deepest, form places of refuge and rest. A morale of persistence has been built up. No party returns without its measurements, and these are carefully made. Forecasting has now passed mainly into other hands, but continuity is assured. Old survivors are few. Of these, the most trusted has just been buried beneath the snow of his beloved Tahoe—Bob Watson, tavern guide, who joined the snow survey when others failed. His sampler, like a trusted sword, lay ready on the porch for the waiting survey as his life ended.

The infancy of snow surveying is now past. The National Research Council through the American Geophysical Union has made the hydrology of snow one of its principal projects, and a com-

mittee of snow has been organized covering the entire snow-bearing portion of the United States and its chief watersheds and representing every phase of snow from conservation to removal, from forecasting streamflow and floods to announcing the ideal places and conditions for winter sports.

At the urging of the Western states, Congress has appropriated funds and authorized a Federal-State Cooperative Snow Survey System there in charge of the U. S. Bureau of Agricultural Engineering. Under the impetus given by snow and floods this past winter in the East, Congress is passing legislation to impound potential flood waters at the heads of the streams. To aid in their better control, snow surveys now centered there will inevitably spread to cover the entire north Appalachian region.

To permit the extension of the snow survey idea under world organization, the International Union of Geodesy and Geophysics, through the International Association of Scientific Hydrology has authorized the formation of an International Commission of Snow, which again through inherent vitality and opportunity has already expanded to include every phase of snow and ice from the evaporation sunpits of the Andean snows to the ice caps of the Polar regions. Practically all the snow countries of the world have joined and the members are world leaders in their subjects and several are headliners. But herein is another saga worth telling later.

As these lines are being written, a request has come from the British-American Himalayan Expedition for a snow sampler to use in connection with its work, thus showing the turn that has come in foreign view-point. The *London Graphic* once paid this compliment to snow surveying: "In the Himalaya, Englishmen climb mountains for sport, but in America for business." As with the English, sport came first in America and, though a new science has arisen, the sense of pleasure and beauty that gave it birth will ever remain to foster it.

CELESTIAL MECHANICS IN THE SIXTEENTH CENTURY

By Dr. W. BURKE-GAFFNEY

PROFESSOR OF ASTRONOMY, JESUIT SEMINARY, TORONTO

In the thirteenth century Saint Thomas Aquinas voiced the opinion: "Although the suppositions of the astronomers seem to explain the observed phenomena, we must not say that they are true; for perhaps the apparent movement of the stars may be explained by some other method not yet understood by men."[1]

Early in the sixteenth century Nicholas Copernicus was convinced that the suppositions of astronomers were false, and excogitated an alternative explanation. He attributed three motions to the earth. It rotated on its polar axis, completing a rotation in twenty-four hours; it journeyed round the sun in a year, with its polar axis inclined to the plane of its orbit; and it turned on an imaginary axis normal to the plane of its orbit, at the rate of one rotation each 25,816 years. The last-mentioned motion is the least spectacular, but the most original. Aristarchus of Samos had thought of the heliocentric possibility in 280 B.C.; Hicetas of Syracuse had favored rotation on the polar axis about 450 B.C.[2]

Nowadays there is a common misapprehension that the teaching of Copernicus gave rise to the question: "Does the sun move round the earth, or the earth move round the sun?" This is not precise. The antithesis of the question is not perfect. The question as to the cause of night and day was this: "Is it the sun circling the earth, or the earth *rotating on its axis?*" Scripture seemed to say the former; Copernicus held the latter.

Copernicus did not consider he was contradicting Holy Writ. Already, in the year 1377, Bishop Nicholas Oresme of Lisieux, whilst canon of Rouen, had shown how the Scripture might be read without deducing that the sun moves.[3] Copernicus was also a canon, and a pre-Reformation canon. He worked out his system about 1506 or 1507—before the Reformation was launched and religious bitterness stirred up. He had no axe to grind. He had not to fend for his living, as Kepler; he was not seeking fame, as Galileo; he was not a highly paid servant of a religiously intolerant king, as Tycho Brahe. He was not wedded indissolubly to one philosophic system; he was a member of no religious order. In a sense he was an armchair philosopher, but a very acute one. He was born of that aristocracy across whose minds the shadows of the future never pass. He studied classics, mathematics, astronomy, medicine and jurisprudence at the universities of Cracow, Bologna, Padua and Ferrara, and took his doctorate in canon law. He practised medicine before taking up his canonry. Astronomy was his hobby. He pooh-poohed the idea of publishing. About the year 1529 he wrote a synopsis of his theory for some who asked him.[4] It was then a time when he might well be hesitant about publishing. In 1517 revolt had been proclaimed in Wittenberg; in 1520 Luther had been excommunicated. But Copernicus had nothing to fear from Rome.

[1] St. Thos. Aq. in Arist. Stag. De Coelo et Mundo lib. Comment., Lib. II, lect. XVII.

[2] "Greek Astronomy," Sir Thomas Heath (London, 1932), xxxvi, xlix and 108.

[3] Pierre Duhem, *Revue générale des Sciences*, November, 1909.

[4] "A History of Science, Technology and Philosophy in the 16th and 17th Centuries," A. Wolf (London, 1935), 13.

His bishop, Giese, had all along approved, admired and urged him to print. In 1533 Johann Albrecht Widmanstetter enthusiastically expounded the Copernican theory to Pope Clement VII. In 1536 Cardinal Schönberg pleaded with Copernicus to benefit science and letters by giving his treatise to the world. The peace-loving canon shrank from possibly provoking controversy. His opinions were already widely talked of and were not on all sides well received.

The Abbot Francis Maurolycus wrote a book "Cosmographia" in 1535. He tells us he completed it "on Thursday 21 October 1535, the day that the Emperor Charles V came to Messina on his return from the African campaign." He published it in 1540. In it he demonstrates, by the customary arguments, that the earth is round and in the center of the universe. Having answered the classical objections, he continues: "And it would not be necessary for astronomers to refute any other principles as regards the earth, if diversity of opinion and human fickleness had not so grown that it is doubted whether one may perhaps believe and say the earth turns on its axis whilst the heavens stay at rest." He deals summarily with this new notion, without mentioning the names of any who hold it.[5] Nor did Luther mention any name when he spoke out his mind on the "new astronomer." "The idiot is bent on upsetting the whole science of astronomy," he said, and denying the Scriptures, for "Joshua bade the *sun* to stand still, and *not the earth*." This was, according to Luther, June 4, 1539.[6] The following month George Joachim Rheticus, professor of mathematics in the University of Wittenberg, came to Copernicus at Frauenberg for first-hand information.

[5] "Joannis Kepleri Astronomi Opera Omnia," Dr. Ch. Frisch, editor. 8 vols. (Frankfort, 1858–1871), VIII, 584.

[6] "Luther," Hartmann Grisar, tr. E. M. Lamond, 6 vols. (St. Louis, 1915–1917), VI, 25.

The canon and the heretic became fast friends. Copernicus let Rheticus write letters descriptive of his system—letters to be published, which they were, at Danzig in 1540, and Basle in 1541. No storm of protest broke: it was to a private correspondent Melanchthon wrote that the authorities should restrain such license of mind.[7] Satisfied with the reception which the *Narratio prima* of Rheticus received, Copernicus handed over his complete treatise for publication. Rheticus was to supervise its printing at Nürnberg. He did, until he was appointed professor at Leipzig in 1542. He then confided the task to Andreas Osiander, a well-known theologian at the university. Kepler, in his "Apologia Tychonis" (written in 1600–1601; first published in 1858), testifies he saw letters which passed between Copernicus and Osiander, in which the former protested that he wished it to be clearly understood that he held the solar system to be in fact as he described it; his theory was not an empty formula.[8]

The first copy of Copernicus' "De revolutionibus orbium coelestium" was delivered at Frauenberg a few hours before its author died, after months of illness, on May 24, 1543. Osiander had inserted a preface, supposed to be by Copernicus, protesting that the system was being put forward merely as a useful hypothesis. Bishop Giese wrote indignantly to Rheticus and to the senate of Nürnberg. But the damage was done. It was generally accepted that Copernicus was the author of the preface. Nothing approaching a public denial was made until Kepler proclaimed the truth in his "Astronomia Nova" in 1609.[9] Kepler had learned of the fraud from a colleague at Nürnberg. His announcement of it was not very public, for his

[7] "History of the Planetary Systems from Thales to Kepler," J. L. E. Dreyer. (Cambridge, 1906), 353.

[8] "Kepleri opera omnia," I, 245.

[9] *Ibid.*, III, 136.

"Astronomia Nova" was printed privately, and not every one who wished to have a copy got one. Nor was it so generous a *geste* as at first sight might appear; for in that same book, on the back of the title-page of which he announced that Copernicus did not hold his theory as a mere hypothesis, he also demonstrated that, if the planets and the earth did move, they did not move in circles, as Copernicus had taught, but in ellipses.

After the appearance of Copernicus' book Melanchthon came into the open and declared (in 1549) it was a sin and a scandal to publish such nonsense.[10] Needless to say, in that God-fearing age there was no great rush to gainsay the pious and learned divine. Besides, to most men the new theory was not sense. The sun not going round the earth! Whoever heard of such a thing! It was patently opposed to the evidence of the eyes. And for the scientists Melanchthon had arguments drawn from the physics of the time to show that what he stigmatized as nonsense was in fact nothing else.[11]

With the sole purpose of defending the principles of Copernicus not a book was written until the year 1584; and not a single astronomer printed an unequivocal and complete profession of faith in them until Kepler did in 1596. Dreyer searched history, and all he could find prior to 1584 were the non-committal *obiter dicta* of two Englishmen and Pierre Ramée's destructive criticism of Ptolemy.[12] Robert Recorde in his "Pathway to Knowledge" (1551) wrote: "Copernicus, a man of great learning, of much experience and of wonderful diligence in observation, hath renewed the opinion of Aristarchus Samius, and affirmeth that the earth not only moveth circularly about his own centre, but also may be, yea, and is, continually out of the precise centre 38 hundreth thousand miles; but because the understanding of that controversy dependeth of profounder knowledge than in this introduction may be uttered conveniently, I will let it pass till some other time." Thomas Digges in his preface to "Alae seu scalae mathematicae" (1573) used the expression of Copernicus that the Ptolemaic system is like a set of heads and limbs taken off different people, which shows that it is not true. He studiously avoided saying that Copernicus had found the truth. (Much later, in 1592, he favored Copernicanism; but this is not to the point at the moment.) Pierre Ramée, in his "Scholarum mathematicorum libri XXXI" (Basle, 1569), was for overthrowing all existing systems, including that of Copernicus, and starting *de novo*.

In 1584 Father Diego de Zuniga, an Augustinian monk and doctor of divinity, of the University of Toledo, took up the cudgels on behalf of Copernicus. In a commentary on Job, published at Salamanca, he endeavored to show that the movement of the earth was in accordance with the words of Scripture. At the time he wrote, the Copernican theory was a matter of purely academic interest. Thirty-two years later, when it was a burning question, his book was relegated to the "Index."[13] That is another story.

Judging from published works, it would seem that the theory of Copernicus was less accepted in the last fifty-seven years of the sixteenth century than was the special relativity theory of Einstein in the second seven years of the twentieth century. Copernicus's "De revolutionibus" was reprinted only once during the century, at Basle in 1566, by the faithful Rheticus. On the other

[10] "Tamen asseverare palam absurdas sententias non est honestum et nocet exemplo."

[11] "Nikolaus Copernicus," Adolf Müller. (Freiburg, 1898), 85.

[12] "Planetary Systems," 346–347; 358–359.

[13] Dreyer, "Planetary Systems," 353.

hand, the "Cosmographia" of Maurolycus was printed four times between 1540 and 1570. And a similar work by Peter Apian, professor at Ingolstadt, ran to three editions comprising fifteen printings between 1524 and 1598.[14] In 1542 Erasmus Reinhold, professor at Wittenberg, published his "Theoricae novae planetarum," which he announced as "an introduction and inducement to the study of Ptolemy." It was reprinted in 1555, 1580 and 1601. In 1549 Reinhold put out a Greek and Latin version of the first book of Ptolemy; and in 1550 an edition of Sacro Bosco's "Sphaera," with a preface by Melanchthon. In 1551 appeared his famous "Tabulae Prutenicae," in the text of which he frequently refers to Copernicus, and calls him "another Atlas or Ptolemy." But, he tells us, the tables were constructed from the observations of Copernicus, Ptolemy and Hipparchus, and, besides his observations, he borrowed nothing from Copernicus except outlines of his explanations (*demonstrationum vestigia*).[15] The "Prutenic Tables" superseded all previous astronomical tables. They were used by every astronomer in the last half of the sixteenth century; they were used by John Field in preparing his "Ephemeris for 1557"; they were used by Father Christopher Clavius, S.J., in the making of his calculations for the Pontifical Commission (1576–1582) for the reform of the calendar. Father Clavius had, in 1570, published a commentary on the "Sphaera" of Sacra Bosco (John Holywood), in which he noticed the theory of Copernicus and rejected it on scriptural and physical grounds. The "Commentarius" of Clavius proved most popular and ran to eleven impressions before the end of the century.[16] Professor Kaspar Peucer, of Wittenberg, in his "Hypotyposes orbium coelestium" (1571) says he passes over the hypothesis of Copernicus lest beginners be offended and disturbed thereby; and, further on, that the absurdity of Copernicus, so far from the truth, is offensive.[17] And Michael Maestlin in his "Epitome Astronomiae" (1582) expounded all the old theories without even mentioning Copernicus.

At the end of the sixteenth century the most eminent astronomer living was Tycho Brahe. When he died, in 1601, Kepler was but a budding sprout; and Galileo had not yet built a telescope. Brahe's work was overshadowed by the discovery of the telescope; but his name is writ in history as the most accurate naked-eye observer of the heavens since the days of Hipparchus (B.C. 140). Brahe jumped to fame through his tireless watching of the Nova of 1572—the variable B. Cassiopeiae.[18] He published an account of his observations under the title "De nova stella" (Copenhagen, 1573). The tract was widely read; its author was in demand throughout Europe for public lectures. One of these, delivered in 1574, was published for the first time in 1610. In it he acknowledged that Copernicus had been justly called a second Ptolemy, who by new hypothesis, deduced by the admirable skill of his genius, showed a more accurate knowledge of the motions of the stars than all who had gone before him. And though, he said, the Copernican theory was somewhat contrary to physical principles, it admitted nothing contrary to mathematical axioms, as did the hypothesis of Ptolemy.[19] Brahe's words bear a striking resemblance to words in the "Quaestiones novae" (Basle, 1573) of that Wursteisen whom Galileo records

[14] "Kepleri opera omnia," VIII, 584, 586.

[15] *Ibid.*, VIII, 587–589.

[16] "Bibliothèque de la Compagnie de Jésus," Carolus Sommervogel, 10 vols. (Paris, 1890–1909), II, Col. 1212.

[17] "Kepleri opera omnia," VIII, 565.

[18] "Katalog und Ephemeriden Veranderlicher Sterne fur 1935," R. Prager (Berlin, 1934), 29.

[19] "Tycho Brahe," J. L. E. Dreyer (Edinburgh, 1890), 74.

as having given lectures on Copernicanism in Italy. In the year 1575 Brahe was considering leaving his country for good to take up work at Basle, when he received a munificent offer from his king, Ferdinand II of Denmark. He accepted. He was presented with the island of Hveen, a magnificent observatory and a life pension. His opinions were fettered. His patron was a stout Lutheran; he had already dispensed with a professor whose orthodoxy had been called in question. But Brahe had plenty of reasons for rejecting the theory of Copernicus. First of all, it was contrary to the teaching of Scripture; secondly, if it were true there should be an annual parallax of the stars—and he, with the best instruments ever made, could detect none—and thirdly an objection of Clavius was unanswerable—a stone dropped from a high tower should land far from the foot of the tower. Brahe was professedly a Ptolemaist until he printed, privately, "De mundi aetherei recentioribus phaenomenis" in 1588. In this he proposed a new system, known to this day as the Tychonic system. It supposed the planets to circle round the sun which traveled round the stationary earth. He declared that he thought of it four years before "as if by inspiration."[20] Another man had been inspired with the same idea eighteen hundred years before—either Apollonius of Perga or another.[21] And one Reymers, also known as Ursus, put out a book, the very same year as Brahe printed privately, expounding a similar theory. Brahe accused him of pirating; but Dreyer, the devoted fellow-countryman and biographer of Brahe, admits that there is not the least proof of plagiarism. The principle of the Tychonic system was so obvious a *via media* between the Ptolemaic and Copernican systems that it would naturally occur to several people. In fact, there was also a Scot, Duncan Liddel, who also claimed to have found it. But why this epidemic of getting away from Ptolemy? Tycho wrote that he was induced to give him up as the result of observations made in 1582–1583 which showed Mars, at opposition, nearer the earth than the sun. Now, Tycho's manuscripts passed to Kepler, and Kepler found that the observations showed nothing of the kind. And, furthermore, in a letter written in 1584 Tycho said plainly that these observations showed Mars to be further away and Copernicus to be wrong! Dreyer admits himself nonplussed by the contradiction.

Tycho and others were straining at the leash of Ptolemy. Why? Is the answer to be found in history extraneous to the history of astronomy? From 1576 to 1591 there was running wild through Europe a philosopher of unbridled brains, who shook the thoughts of the times whilst scattering the seed of modern philosophic doubt—Giordano Bruno. Bruno was born at Nola, near Naples, of poor parents, in 1548. As a boy he aspired to enter a religious order. His extraordinary intellect compensated for his lack of wealth. At the age of seventeen he donned the habit of St. Dominic. The young religious was noted for his lively imagination and his restless spirit. He read indiscriminately; he formed a profound aversion for Aristotle and Scholasticism. In spite of this he was ordained priest and exercised the priestly office in various places. From scepticism he passed to open heresy. It was related at Rome that he cast doubt upon the possibility of a Holy Trinity and the divinity of Christ. He was called there to clear himself. He proved recalcitrant and was threatened with trial by the Inquisition. He sought safety by flight, 1576. He settled in Geneva, where, to gain admittance to the university, he professed his faith in Calvinism. In 1579 he was jailed for

[20] Dreyer, "Planetary Systems," 356.
[21] Heath, "Greek Astronomy," lv.

defamatory libel against a professor of the university. Upon his release he shook the dust of Geneva off his feet, and became a sworn enemy of Calvinists.[23] At Toulouse he obtained a chair of philosophy, assailed Aristotle for two years, and added to his munitions by studying the new astronomy. He then moved to Paris, devoured the "Ars generalis" of Lully, and became an authority on mnemonics. He obtained a special professorship, in philosophy, and wrote on philosophy. He also wrote, at this time, his comedy "Il Candelaio," which rates among the most obscene plays ever written.[24]

In 1583, when France was in a turmoil, Bruno crossed to England, where priests were hanged. During his sojourn he flattered the Virgin Queen, and wrote. Amongst other books, he wrote "La Cena delle Ceneri," the popular attack upon the Ptolemaic system; it was published in Paris in 1584, the year in which Tycho Brahe got his "inspiration"; and the year in which the French historian Jacques Bongars visited Brahe at Uraniburg.[25] Bruno had been refused permission to lecture at Oxford. In his "La Cena" he turned aside from his main purpose to express himself on that university; it was "a constellation of the ignorant, pedantic and obstinate; and a mixture of donkey and swine." In 1585 he was back in Paris, and sought reconciliation with the Catholic Church; he refused to pay the price—return to his order. He left France for Germany, and passed from city to city. At Helmstadt he was denounced by the Lutheran superintendent as an "assassin of souls." At Wittenberg he did well, praising Luther; but the tide turned, and in 1588 the Calvinists drove him out. At Frankfort, in 1591, he received an invitation to Venice. One Giovanni Mocenigo wished to learn the mnemonic art and how to read thoughts. The following year his pupil-host handed him over to the Venetian Inquisition. An attempt has been made to explain this inhospitable act by linking the facts that his host had a handsome wife and that the charge laid against him was not only heresy but also that "he had taken great pleasure in women." However this may be, the Roman Inquisition demanded, and obtained, his extradition, on the grounds that he had long been "wanted" there, as an apostate priest and a fugitive monk.. He was held for eight years and given every opportunity to recant, before being burned, as a traitor to his faith, February 17, 1600. "The fate of Giordano Bruno," says Dreyer, "can hardly have been influenced by his advocacy of the earth's motion, for he had to set forth a sufficient number of startling ideas to provide stakes for many scores of heretics."[26] And the best that A. V. Butler can say of him is that: "as an apostate monk, a loose liver, the wielder of an acrid pen, he had at least given provocation, and caused something of a scandal in several countries of Europe."[27] He was, of course, no astronomer.

Whilst Bruno was at Wittenberg there came to sit at Michael Maestlin's feet, at Tübingen, a poor scholar of scarcely seventeen summers—Johann Kepler. Michael Maestlin's "Epitome Astronomiae" was reprinted that same year, 1588. As has been said, it was thoroughly Ptolemaic, with not a word to say for Copernicus. But Kepler tells us that Maestlin taught Copernicanism. It would seem to have been the fashion of the time to say what one thought, but to write what one ought. Maestlin's teaching Copernicus was a happy fault.

[23] Pastor, "The History of the Popes," XXIV, 204.

[24] Pastor, XXIV, 205.

[25] Dreyer, "Tycho Brahe," 157.

[26] "Planetary Systems," 416.

[27] "The End of the Italian Renaissance," *The Cambridge Modern History*, III, 466.

His pupil was convinced of the truth of the Copernican system. When elected to the position of provincial mathematician at Grätz (1594), he took it for granted, and gave much thought to the questions: Why did the all-wise Creator place the planets as he did? Why were the dimensions of the orbits what they were?[28] Finally he found what he believed to be the true solution; and he carefully noted the date of his discovery —a date which he expected would go down in history, 9/19 July, 1595. He wished to rush into print; he wanted to tell the world that in designing the universe the Almighty had used the number, and relations between the dimensions, of the five regular solids—the cube, tetrahedron, dodecahedron, icosahedron and octahedron, and that his, Kepler's, discovery was consistent with Copernicus's theory. But he also wished to publish under the egis of the University of Tübingen, and this was not so easy; the Copernican theory was not in good odor among the theologians there.[29] At length, however, his "Prodromus" was off the press (December, 1596); it has claim to be the first and only publication of an astronomer in the sixteenth century to unequivocally defend the Copernican system. As fates would have it, it was this same Kepler who was (thirteen years later) to prove that the Copernican theory was false, that the earth and planets did not move in circles round the sun, but in ellipses.

If Kepler's "Prodromus" did nothing else, it served to introduce him to the notice of the scientists of the time. It led to his holding correspondence with Tycho Brahe; and after Brahe had become imperial mathematician to the Catholic Emperor Matthias, at Prague, Kepler joined him (in 1600) as assistant. In 1601 Brahe died; Kepler succeeded him as imperial mathematician and received his manuscripts. If, in after years, he renovated the whole science of celestial mechanics, it was in large measure due, as he acknowledged, to the thirty tireless years of work of Brahe. Without Brahe's observations Kepler could never have achieved what he did. Brahe must not be forgotten. "Si fiers que nous puissons être des succès d'aujour'hui, nous ne devons pas oublier ceux qui, par leur travail opiniâtre et leur intuition géniale, ont ouvert les voies sur lesquelles nous continuons à avancer."[30]

[28] "Kepleri opera omnia," I, 106–109.
[29] *Ibid.*, I, 19–24.
[30] Louis de Broglie, *la Revue d'Optique théorique et instrumentale*, 79, 1927.

AN ADVENTURE IN ETYMOLOGY

ORIGIN AND MEANING OF SOME ANIMAL NAMES

By ERNEST INGERSOLL

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THE first-comers from Europe to this part of the world were eager to get not only a knowledge of its extent, climate and geographic features, but also of its productions. French and Basque fishermen before the time of Columbus knew of the cod-fishery in the waters about "New-Found-Land," and were vaguely acquainted with the natives of its shores and with certain fur-bearing animals. Later, French navigators explored the valley of the St. Lawrence with an eye to extending their king's dominion and wealth and the influence of the Catholic church, but paid only careless attention to the plants and animals they encountered. Much the same imperial notions and objects actuated the Spanish conquerors, who, in the sixteenth century, penetrated the region north of Mexico. Their reports contained small mention of natural products, for their efforts were spent on hunting for mines of gold or gems or on the Christianizing of whatever human inhabitants were discovered.

From the records of these primitive French and Spanish expeditions we shall gain therefore little help in our search for the origin and significance of the vernacular or "common" names of North American mammals—to which this essay is devoted. Our debt, on the contrary, is with few exceptions owing to the English-speaking adventurers who later obtained possession of the vast space between St. Lawrence Bay and the Gulf of Mexico. They were intent primarily on neither gold nor religious missions, but on trade and self-sustaining colonies. These explorers, planters and traders—never crusaders—were wide awake to sources of wealth and eagerly examined every path to that end; hence their reports and letters to the rulers and people of the Old World abound in information as to navigable rivers, useful forests, tillable land and the like, and frequently included observations and lists of trees, animals, fruits, etc. Each "home"-going ship carried samples of almost everything noticed that was transportable.

In this way, from the very beginning of occupation along the Atlantic coast, specimens of many American mammals, birds and other creatures fell into the hands of European naturalists of that time (including Linnaeus), each specimen "marked for identification" as fairly as the sender knew how. It must be remembered, however, that these early residents on our shore—whatever their virtues otherwise—were ignorant not only of zoology, but most of them were townsmen lacking an English farmer's scanty knowledge of such things. When they saw a bird or flower that somehow suggested what they had happened to have known in the "old country" they flung that name upon it—and made some dreadful mistakes! Therefore the descriptions as to haunts and habits attached to their specimens were likely to be indefinite, and the exact source of each was rarely indicated; at the same time the European classifier had equally hazy notions of American geography. Thus honest ignorance erected a difficulty in the way of the word-tracker who

is struggling with what Dr. Elliott Coues once called "ornithologicalities."

Having disposed of these facts and reflections I will now turn to the anticipated topic of this paper, namely, an examination of the why and wherefore of the English-American names of our more or less familiar mammals. Any zoological group asked to account for its names will yield interesting paths to be traced, and a fair trial of the sport may be made with the comparatively short list of North American mammals, which we may conveniently investigate alphabetically. The first name to respond will be *Antelope*, meaning the pronghorn.

Here our feet strike a stumbling-block at the very start, for, in addition to the fact that our pronghorn is not in that class of old-world ruminants, but only looks like it, we soon learn that "antelope" is an antique Greek noun that has worried etymologists from 'way back. It seems to have been a synonym of "gazelle" and shared with that term the sense of bright-eyed—a feature poets have always celebrated. In proper classification our pronghorn (a model of suitability in names!) stands by itself as an erratic cousin of the goats; and the early Spanish explorers called it simply a little goat, which accounts for the name cabrée or cabrit (Spanish *cabrito*, a kid) still heard along the Mexican border.

At the head of the words indexed under B is the *Badger*. No fur-bearing animal in the world, perhaps, is more widely known than this—at least by name. This matter of its label is a curious one, especially as the shaggy little beast has two names, an older and a newer one; in fact, the American species, differing from the historical European one mainly in coloring, is alone entitled to the latter. Since English speech became organized the British have called this animal "broc," apparently a Welsh word carrying the idea of grizzle or grayness, suggested by the color of its coat. This term still holds good among country people, but long ago British sportsmen translated it and spoke of the animal simply as "the gray." In fact, the book-name badger was not introduced into English speech until the sixteenth century, and most dictionaries still instruct us that it means nothing more than "wearer of a badge," referring to the white stripe down the creature's face; and they try to support this explanation by quoting another almost forgotten designation, "bauson," meaning piebald.

Opposed to this, Dr. W. W. Skeat and his school of etymologists declare that the name badger came into Britain (and thence over here) by way of Italy and France, supporting their view by showing such changes in letters, pronunciation and so forth as obscure the history of hundreds of our words of foreign birth. The French called wheat *ble*, which represents the core of the Latin *ablatum*, the ancient term for "corn" (cereals), particularly wheat. Roman grain-merchants seem to have been so distrusted by their customers that they were dubbed "corn-stealers," and so were classed with the badgers that damaged Italian farmers' crops and were hated as pests. The Italian epithet for both the mischievous little beast and the cheating corn-dealer passed into old French as *bladier* and soon crossed the English channel. A more or less rude Britisher of former times, as now, would pronounce it "bladger," and the over-easy dropping of the letter *l* left it sounding as badger. Really this reasoning is fairly straight. By a precisely analogous loss of an *l* our speech has acquired the animal-name *Bat* from the *Icelandic* Blatta, a flutterer; Germans call bats flittermice. In modern French, badger is *blaireau*—a word used to-day in Quebec, usually sounding, however, more like "braro."

Queer evidence that we have derived the English word badger and the stigma involved with it from Roman grain-market talk is found in the writings of John Strype, an English historian of the early eighteenth century, who recorded much about London and its bridges. Famous old London Bridge, begun in the twelfth century and lasting 400 years, had a score of arches to sustain its roadway and picturesque burden of houses and temples. Strype says that in Queen Elizabeth's time much complaint was heard in London over the extortionate prices asked by both millers and dealers for flour and meal. To check this oppressive condition the magistrates set up in several of the arches of the bridge water-mills for public benefit. "The profit of these mills was that whereas in time of dearth the common people could not have their corn ground under four, five or six pence the bushel, and many times could not have it ground at all in a short space, by means whereof the people were constrained to buy meal in the market at such prices as the seller himself would demand, this would be remedied by these mills. Also the badgers, or meal-sellers, advanced prices as they listed." Here history shows the former food-merchants and millers in the same bad light as their Italian predecessors, and having the same spiteful nicknames—Badger! Cornstealer!—shouted at them on the banks of the Thames as used to be hurled at them in market-places beside the Tiber; and in reality the market-men seem to have deserved their bad reputation far more than the animal did his.

Bear and *Beaver* are what etymologists call Old High German words, so old that why they took that shape or what were the animating ideas that led to it are quite beyond call. Most of the different bears are so simply named by color or place that no explanation is required, but a word may be pertinent about our "grizzly." This master of the western mountains was brought to scientific notice by the Lewis and Clark expedition in 1804. Captain Lewis gave it the name "grizzly," which means having a grizzled coat; but he also wrote "grisly," which (if intentional) implied the high respect he had for its power and prowess. Doubtless this "grisly" view led Ord to give the dreaded animal the Latin specific label *horribilis*—horror inspiring—rather than the hoary hue of its coat (as if "frosted" with white hair-tips), since it was soon learned that its prevailing color is some shade of brown, as in all other northern bears except the polar species. Indeed it seems to be agreed that the root-notion behind the modern word "bear," in its various forms between India and Iceland, was a prehistoric expression of the creature's characteristic appearance—it was the "Brown One." A substantial reason for believing this explanation is the common way of speaking of a bear as bruin, which is simply the Dutch adjective "brown." The nickname "Old Ephraim" given to this bear by western hunters was doubtless in reference to the character of warrior belonging to that fighting son of Joseph and the tribe he founded in Judah's Canaanitish wars; but why the frontiersmen styled him "Moccasin Joe" is unexplained, unless it was suggested by its footprints.

Beavers, like the bears, owe their very ancient name to brownness.

Bison comes next in order. This has been the label, under various spellings, as "bisonte" and "wesant," etc., of the wild forest beast the Romans called "aurochs," learned from the Germans, and which we popularly but erroneously know as the *Buffalo.* Spanish explorers of our Southwest found it in northern Mexico and adopted the Indian name

"cibola," soon forgotten. French adventurers who met the animal in southern Ontario spoke and wrote of it merely as a native sort of beef-cattle (*le boeuf*); the first English colonists learned of its existence under this name, modified the French word into "buffle," and this soon became altered into buffalo. It is unfortunate, as has been said, that most of our more conspicuous mammals and birds were first seen and reported by persons ignorant of zoology and reckless of diction.

A good example of such misnaming is suggested by the word *Cony*, nearly obsolete on this side of the Atlantic. It designates in Britain the common small burrowing leporid to which alone the name properly belongs. Newcomers into New England saw our little cottontail and dubbed it "cony" (as later men did the pika), not observing that it did not burrow and was really a small hare; and so we call all our hares rabbits. Another misnomer is the book-name *Civet-cat*, although it is neither one nor the other, but is in reality a distant cousin of the raccoon properly named "cacomistl" (Mexican Indian). Fortunately the people of Arizona, who find it an agreeable pet, have suitably named it "ringtail." This is a neat and pretty creature built like a 'coon but looking more like a fox; so the first scientific describer hunted up a Greek word for fox and added "bassarisk" to his index.

Coyote, our red, prairie or "barking" wolf, brought with it from Mexico its Anahuac (Indian) name coyotl; this in American speech soon lost its terminal *l* and acquired an *e*, which made the proper pronunciation coy-yó-te, not kýyote, as most plainsmen perversely sound it.

Four other mammal names have the initial C—caribou, carcajou, chicaree and chipmunk. *Caribou* must be credited to Canadian French, and probably was taken from some eastern Indian dialect, perhaps of Newfoundland. *Carcajou*, the wolverine, but often mistakenly applied to the lynx or the puma, is another French-Canadian borrowing, as we shall see when we come to "wolverine." *Chicaree* is a nickname for the red squirrel used by Audubon and other early writers, suggested doubtless by the querulous chatter of this excitable little rodent. Imitative, too, are *Chipmunk* and the forgotten "hackee" for the striped spermophiles now often, in the Northwest, confused with other species as "gophers." *Cougar*, a name of Brazilian origin, is still prevalent in the southern United States for our big forest cat or "mountain lion," which will be further considered when we come to *Puma*.

Deer is a term little changed from remote times in the north of Europe, signifying wild—a wild thing. Originally it seems to have covered any wild animal, and this broad sense has lasted, at least in English, until comparatively recent times—witness Shakespeare's, in the familiar line in "King Lear," act 3, "But rats and mice and such small deer."

When or how the word came to be restricted to the family of woodland ruminants we call deer is obscure. My impression is that the change is associated with the rise of the oppressive game-laws enforced by the kings and feudal lords of England in the twelfth and thirteenth centuries, as indicated in history and in such traditions as the ballad of Robin Hood. Under such royal influence and laws the red deer and the roe became the only "game" sportsmen considered worth while, and popular opinion and language gradually followed. I may here quote in part remarks by the learned author of Bell's "British Quadrupeds," who wrote:

The derivation of the English names applied to the deer tribe is interesting, showing how completely the simple Saxon words of the stout yeomen and outlaws ousted the Norman-French terms used by princes and barons. Thus deer is the Anglo-Saxon *deor*, its primary meaning signifying simply a beast (German *thier*, Greek *ther*, Latin *ferus*). Stag originally meant a male animal of any species, as in the Icelandic *steggr*, a male, and the Scotch *staig*, a young horse. *Heort* (hart) and *hind* are also Saxon. The fallow deer takes its name from its color (Anglo-Saxon *fealo*, yellow). "Buck" is probably from the Teutonic *bocken*, to strike—an animal which butts with its head; hence it has become the general name of the male beasts of the chase, even including those which have no horns, as the hare and rabbit. "Doe" seems to contain the sense of tameness, harmlessness, and "fawn" is thought derived from an old French word for offspring; but more modern students refer it to an Icelandic source.

The common forest deer of the eastern states has been styled Virginian from the beginning, but when found in the West it was naturally christened whitetail, from the conspicuous snowy "feather" it elevated when startled, or willow deer from its wise habit of sticking to the shelter of the willow-bordered watercourses in the open regions there. In contrast, the larger plains deer is known locally as blacktail, mule or jumping deer, in reference to characteristic features and gait. Two other American species of this family call for more special notice—the wapiti and the moose.

Moose is one of the few names of American animals accepted from the Indians, and is a near reproduction of what the Algonkin-speaking tribes called this big, flat-horned deer, and came into use as early as 1632. The first Frenchman who attempted to settle about the Bay of Fundy mentioned in their records the first deer they saw by queer provincial names, but as to the moose, a creature utterly new to their eyes, they could do no better than to call it simply *l'original*, showing that they failed to recognize it as the counterpart of the great elk of Scandinavia, of which very likely they had no knowledge. Early British colonists, however, recognized the resemblance. In his "New English Canaan" (London, 1632) Thomas Morton, listing the animals of New England, says: "There are in this country three kinds of Deares. . . . First, I will speake of the Elke, which the Salvages call a Mose; it is a very large Deare, with a very faire head & a broade palme, like the palme of a fallow Deares horne, but much bigger."

The Virginians fell into great confusion between these two deers, a confusion of which we are not yet quite rid, for when on the James River they first saw or heard of a "great" stag they assumed that it was the elk of northern Europe, when in truth it was our wapiti. I had made considerable search to ascertain where and how that mistake first happened when I discovered that Ernest Seton had answered my question in his magnificent book "Northern Mammals." Seton had found that in 1605 Captain George Weymouth recorded in his "Voyage to Virginia" "that the savages sign unto us a certain deer with horns and broad ears which we take to be Olkes." As Seton tells his readers—

This is the earliest known use of the word "olkes" or "elk" with reference to the American. It appears in the latter form in 1650, when Virginia is credited not only with abundance of deer, but also with "elks bigger than oxen." [This information Seton derived from the Force Tracts, Vol. 3, No. 11.] . . . After this date the number of travellers increased in America, and their accounts frequently included descriptions of "the great stag that was of the bigness of a horse."

Seton then gives two interesting quotations from two of these early writers, as follows:

Thus Mark Catesby, in 1731, remarks on "the stag of America . . . they usually accompany the Buffaloes, with whom they range in droves

in the upper and remote parts of Carolina, where, as well as in our other colonies, they are improperly called Elks. The French in America call this beast the Canada Stag. In New England it is known by the name of the Gray Moose to distinguish it from the preceding beast, which they call the Black Moose. . . .

In March, 1806, Dr. B. S. Barton published "An Account of the Cervus Wapiti or Southern Elk of North America." He remarks: "As the Elk has not to my knowledge been described by any systematic writer on Zoology, I have assumed the liberty of giving it a specific name. I have called it Wapiti, which is the name by which it is known among the Shawnees or Shawnese Indians. . . . This animal is generally known in Pennsylvania and in other parts of the United States by the name of Elk."

Mr. Seton assures us that this is the first use in print of the word "wapiti," so far as he knew, and probably he is right.

Resuming our progress, the next item is *Gopher*. This is our spelling of the French word *gaufre* (honeycomb), which was used by the French pioneers in Louisiana to indicate a creature that made clustered burrows in the ground, fancifully likened to the cells in a honeycomb. It was also applied (badly) to the large southern tortoise and burrowing kind of snake; but the present familiar use of the term did not begin until about 1850, when Kennicott reported that the "pouched rat" was known as "gray gopher" among Illinois farmers. Since then almost any ground-squirrel is a "gopher" in the West.

Groundhog is the senseless name, originating apparently in the southern states, for *Woodchuck;* it persists through the silly newspaper preservation of a still more senseless weather-proverb, imported by German immigrants in Pennsylvania.

Under the initial H come such remotely antique generic words as hare, horse, hog and the like. *Hare* has descended from the Sanscrit verb "to jump." *Hog* may be guessed at; my guess is that it is simply an imitation of the animal's grunt. *Horse* contains no suggestion of any equine voice. The horse seems to have been distinguished among the beasts in the primitive German forest as "the running one"; but whether this apt recognition-mark in words originated with the first Eastern immigrants, and hence belongs wholly to the European forest pony, is not clear.

Jaguar, the big spotted cat of tropical America, a century ago common in eastern Texas and the swamps of Louisiana, preserves in English its name among the Guarani Indians of Brazil, literally signifying "big dog." Although so leopard-like, it is called "the tiger," except in Mexico, where it is known as the *ocelotl* of the woods.

Next comes *Lynx*. As we spell it the word is pure Latin, behind which is a long ancestry carrying the idea of light (Latin *lux*). The application in this case appears to be in the sense of brightness, for this animal seems to have been regarded by the ancients as "the cat with the shining eyes." All cats' eyes glow by reflected light under favorable conditions, and the eyes of the large species shine more than others. Hence the big fierce lynx has become especially noticeable in this respect. Also in another way. All cats see well in gloom and have been popularly credited with ability to sparkle in total darkness, which of course is impossible. On the principle that a big cat has more power than a small one the belief grew in the old fable-loving times that the lynx could look through opaque objects. Hence the original credit became extended from shining eyes to all-seeing ones, and we speak of any animal or man as "lynx-eyed" because ancient fabulists said so. The old Canadian French hunters knew this wildcat as *loup cervier* (deer-wolf), commonly pronounced "lucivée."

Mammoth and *Mastodon* are scientific

terms not generally well understood. The extinct elephants named mammoths are not so called with reference to enormous size, but from a Yakut word (in Russian *mammot*) meaning a burrower, because when the first specimens were discovered entombed in the frozen cliffs of Arctic Siberia, the Yakuts reasoned that when alive they had lived underground—in fact, were huge moles! In truth, the mastodon owes its scientific name to a peculiarity in its dentition.

Mole seems to be a shortened form of the Middle English "mould-warp," meaning an animal that throws up the mould or soil. Scotsmen still speak of the mole as "modewarp," as frequently occurs in the poems of Burns and other writers.

Moose is one of the few animal names we have adopted from the Indians and closely follows in pronunciation the Algonkin (Abenaki) original, the meaning of which is "wood-eater."

The name *Muskox* is a misnomer in a double sense as applied to this Arctic relic of the Pleistocene. It is not at all an ox, but stands by itself between the cattle and the sheep, as indicated by its generic label *Ovibos;* furthermore, it is not at all musky. Stefansson had much experience with it in the Western Arctic islands, and agrees perfectly with Sverdrup and other competent observers that no taint of musk belongs to this fine animal either when alive or as edible flesh. Stefansson recommends dropping this deceptive name, and calling these animals *Polar Cattle.*

The *Muskrat,* on the contrary, has a good and suitable name, for it is both a rat—a sort of huge meadow-mouse—and its pelt, at least, is unpleasantly odorous; nevertheless the animal does not owe its name to these characteristics, but to the wide-spread designation of it in the Algonkian language, varying dialectally in the various tribes. Captain John Smith reported it in 1616 as "musquassus" in the Powhatan speech of Virginian Indians; the Crees of the Northwest said "muskquessu," meaning "it is red"; Morton (1632) learned it from the Abnaki as "muskewashe"; and the early Canadian trappers pronounced it "musquash," which the Yankees borrowed and have never forgotten, but unhappily has been displaced by the unimaginative form "muskrat"!

Under the initial *O* are to be listed here three mammal names—ocelot, opossum and otter. *Ocelot* calls to mind that beautifully mottled cat of Texas and southward, whose name is a shortened form of the word *ocelotl,* which Mexican Indians are said to apply to wildcats generally, but specifically to *el tigre,* which we know by its Brazilian epithet as jaguar. Lesser cats are distinguished by descriptive adjectives. The ocelot, as we know it, is termed "field-jaguar" because it chooses to live in open hunting-grounds, while "tigre" is a beast of the heavy forests and the mountains.

Ask any group of persons to tell you the origin of the name *Opossum,* and the answer will probably be that it came from Australia, whereas the truth is quite otherwise. This queer little beast was an astonishing novelty to the colonists at Jamestown, Virginia, and its peculiarities were described at some length by Smith and Heriot as early as 1610. They spelled its Indian name *opassum.* When, many years later, the zoology of Australia came to be examined, animals of the same kind were found there, and the Virginian novelty and its native name were recalled; soon all the smaller Australian marsupials came to be known as opossums.

Pekán, our big northern marten, was adopted as a distinction very early from the Abnaki Indians by the Canadians and was first recorded in English in a translation of Charlevoix's "Travels"

about 1820. Few of our mammals have so many aliases. The first specimen of its skin sent to a naturalist in Europe unfortunately carried the label "fisher," and this clung to it in the books, although wrong, for this marten does not catch fish as a part of its habitual fare, although it readily steals and eats one when it finds it serving as bait in a trap—a common practice in Quebec. Hunters also speak of this able weasel as "black fox" or "black cat," which are excellent examples of the sportsman's usual carelessness in language. In most books it is indexed as Pennant's marten, because the first skins to reach Europe and become classified were sent from Canada by that exploring naturalist about 1775.

Pika, the timberline "cony" of the Rocky Mountains, inherits this Siberian name from a similar Alpine species so called in eastern Europe; but our pika is more commonly known as "little chief hare"—an amusing nickname given it by the Indians of northern Canada, and introduced to us by Richardson.

Porcupine, that is, "spiny pig," calls for no remark beyond the fact that it is substantially the same in all the Romance languages. Father De Smet, the famous missionary to the Indians of the upper Missouri region, says that his parishioners called it "prickly beaver." They explained that in old times porcupines and beavers were brothers and lived in company; but the beavers became so disgusted with the former's indolence and dislike of water that one night they left the sleeping camp to their lazy friends, and ever since the two have dwelt apart.

Porpoise is merely an old French word meaning "sea-pig," replaced in modern French by *marsuin*, which is a gallicized form of the German name *meerschwein*, sea-pig again. So the slow and bristling porcupine, carefully avoiding water, and the sleek and agile porpoise, which lives in it, are, etymologically, "sisters under the skin," to borrow Kipling's striking phrase.

This brings us to *Puma*, our own peculiar beast of prey. This is its native name in Peru, introduced to us, I think, by the Swiss naturalist and traveler Tschudi about 1840, and now generally adopted by zoologists. This fine, self-colored cat is remarkable as a species for its distribution from Maine and British Columbia to Patagonia, and also for its many names. It seems to have been first encountered in what is now the United States by the early settlers in New England, who considered it "panther," which, by the way, is an old Greek name for the leopard. Yankee hunters pronounced the word "painter," as is perpetuated in Fenimore Cooper's novels of frontier life; and they also called this greatly feared animal "catamount" and "carcajou"—the latter a mistake, for that is the French-Canadian name of the wolverine. In the southwestern states this cat is usually referred to as "cougar"; but everywhere else in the West it is "the mountain lion." In view of this confusing diversity it is a fortunate circumstance that the short, easy and well-born name *puma* has come into general use.

This somewhat discursive essay may end with a few notes under W, X, Y and Z. *Weasel* and *Wolf* have been current so long in our speech that nothing but guesses at their philological birth remains. Any one who looks at a *Weasel*—how slender and shriveled its body seems—must think of it as "wizened"—its origin? *Whistler* is merely a translation of *le siffleur*, naturally given to the Rocky Mountain cousin of our eastern woodchuck by the French fur-trappers of old times because of its whistling shout. The word *Woodchuck* has been derived from

the name of the animal among the northwestern Cree Indians and has no reference to the forest or anything meant by "chuck."

The name *Wolverine* has been subjected to much unsatisfactory inquiry. That brilliant scholar, Dr. Elliot Coues, devoted two pages in his closely studied treatise on the fur-bearing animals to this hated beast of the Canadian wilderness. The Cree Indians called it *quickhatch* and the French Canadian trappers *carcajóu*, why is unknown. To add to the confusion our patron saint Linnaeus, after examining his first skin, gave it the Latin specific name *luscus*, one-eyed, because the specimen happened to have lost an optic in the process of killing or skinning, just as he named the first bird of paradise he received, *apoda*, footless, not realizing that Malays cut the legs off the skins of these birds, destined to the feather-market, for convenience in packing. The result of Dr. Coues's investigation was the final sentence, "I have no idea what the meaning of the term may be."

Not liking to end this pleasant discourse on so dolorous a note, let me return for a brief moment to that beautiful group, the cervine deer. Surely, of all this family none is finer than our noble stag, the wapiti. It is most fortunate that this splendid deer has inherited from a purely native source a really distinctive designation, *Wapiti* (white-rumped), a name so good that it has been adopted not only by ourselves in place of the erroneous "elk," but also by naturalists and sportsmen abroad as an appropriate and convenient means of separating in a casual way the Asiatic forms of the species from those of Britain and eastern Europe.

SCIENCE, POPULATION AND SOCIETY[1]

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A LARGE number of theories purport to trace some relationship between population density and the rate of scientific and technologic advance. Most of these assert that there is a positive correlation between high population density and scientific progress.[2] Paul Jacoby has even attempted to reduce the relation to a formula. He maintains that since the frequency of geniuses and of their inventions is directly related to density of population and urbanization, one may formulate the expression of a law, $u = f(xy)$, in which density = x, urbanization ratio = y and the frequency of genius = u.[3] According to Adolph Coste, one of the most extreme of these theorists, "the inevitable increase and the progressive concentration of populations . . . is the determining condition without which inventiveness could not be exercised."[4] Feldhaus, Cornejo and Vierendeel have expressed the same opinion.[5]

There are several obvious reservations to this theory. In the first place, as these theorists' own data frequently show, this correlation is far from perfect. In so far as the variable of population density has

[1] The author gratefully acknowledges the financial assistance provided by the Harvard Committee on Research in the Social Sciences.

[2] A survey of many of these theories is to be found in P. A. Sorokin, "Contemporary Sociological Theories," pp. 388–412. New York, 1928.

[3] Paul Jacoby, "Études sur la Selection Chez l'Homme," pp. 542 *ff*. Paris, 1904.

[4] Adolphe Coste, "Les Principes d'une Sociologie objective," pp. 102–3. Paris, 1899.

[5] F. M. Feldhaus, "Die Technik der Antike und des Mittelalters," p. 25. Potsdam, 1931. M–H. Cornejo, "Sociologie générale," Vol. I, p. 415. Paris, 1911. A. Vierendeel, "Esquisse d'une Histoire de la Technique," Vol. I, pp. 11–12. Paris, 1921.

been dealt with in an atomistic and mechanical fashion, the fact that the correlation is concretely absent in many societies casts doubt on the proposition. Thus, Levasseur's criticism that certain provinces in China with a higher density than some areas in France should show more inventiveness than the latter if this theory were valid, applies only if density is treated as an *isolated factor* which *concretely* evokes the same result, whatever the social and cultural context. But, as will become evident, if population density is considered analytically as but one social element in a complex configuration and if its significance is correspondingly circumscribed, such strictures are no longer applicable.

In any event, a mere indication of this correlation does not by any means satisfactorily exhaust the problem. There still remains a description of the mechanisms and processes whereby this association is effected. In brief, what are the characteristics of a high population density which are apt to lead to increased technologic and scientific development?

The answers of the demographers to this question fall into two categories: an increase of the need for new inventions and an intensification of social interaction. The concept of sheer need as a precipitant of invention is very widely accepted despite the obvious limitations of the notion. Thus, W. I. Thomas has popularized the conception that a "crisis gives rise to invention."[6] This represents the familiar fallacy of generalizing a partial truth. Although, as Rossman has indicated, invention in military fields frequently responds to the stimulus of war needs,[7] and the *Atlantique* disaster resulted in inventions for the prevention of fire at sea, and the great flour-dust explosion in Minneapolis mills (May, 1878) resulted in *thirty* effective inventions for preventing the recurrence of such an accident,[8] it is equally true that a multitude of human "needs" have gone unsatisfied throughout the ages. Moreover, countries which are often considered to be the most needy of invention, such as Amazonia and India, have relatively little invention.[9] In the technical domain, "needs," far from being exceptional, are so general that this notion explains little. Each invention *de facto* satisfies a need or is an attempt to achieve such satisfaction.

Another consideration is pertinent. "Need" is an elliptic term which in the contexts considered always implies "realization or consciousness of need." In other words, where an observer from a culture which has a long tradition of attempts to improve material welfare and to control nature may often detect a "need" in another society, that need *may not exist* in terms of the values current in the culture which he is observing. This is the same kind of fallacy which is often implicit in the use of the concept, "culture lag," where it is maintained that a given situation "calls for" (*i.e.*, has a "need of") certain cultural changes. Such statements are valid only within certain value-contexts which *assume* that changes of a specific type are needed to effect an *imputed* goal. But it is only when the goal is actually part and parcel of the culture under consideration that one may properly speak of a need directing inventive interest in certain channels.

The fact is that need, in itself, is not sufficient to induce invention but acts only as a precipitating and directive

[6] W. I. Thomas, "Source Book for Social Origins," pp. 13–26. Chicago, 1909.

[7] Joseph Rossman, *American Journal of Sociology*, 1931, Vol. 36, pp. 625–33.

[8] P. B. Pierce, *Transactions of the Anthropological Society of Washington*, 1885, Vol. 3, p. 165.

[9] S. C. Gilfillan, "The Sociology of Invention," p. 48. Chicago, 1935.

influence if the social context is one which places a high value upon invention, which has a long history of successful invention and which customarily meets the given needs through technological invention rather than by other means of satisfaction. Thus, as Professor Sorokin points out, certain economic pressures may be met by migration of "surplus" population, by war, plundering and the like (as contemporary events illustrate). For example, military technology was substantially undeveloped by the Romans, their military supremacy resting primarily upon discipline. Thus, a need may be met through non-technologic means. But given the tradition of meeting these needs through technologic innovation, a tradition which is thoroughly ingrained in our Western culture; given the prerequisite accumulation of technical and scientific knowledge which provides a basic fund from which to derive means of meeting the given need; and it can be said that in a limited sense, necessity is the (foster) mother of invention.

Once these reservations are fully appreciated, it becomes evident why increased population density, by evoking new needs, does not inevitably facilitate invention. For this process to become effective it is necessary that the society be of the specific type we have described, with cultural values and accumulated civilizational products which not only make possible but largely determine an advancing rate of technologic development.

The second process through which population density is asserted to provoke advance in science and technology is that of increasing social interaction. But this very statement indicates that population density *as such* does not bear any uniform relation to scientific development, since variations in the degree of social interaction may be effected through means other than growing population. For example a sparsely settled area with highly developed means of communication and transportation may have a much higher degree of social interaction than such densely populated countries as China. Hence, since population density is in this sense an accidental concomitant of social interaction and indirectly of marked scientific development, it becomes an avowedly tenuous factor in such development.

Increasing population density may thus facilitate the advance of technology and science primarily in two ways: first, by evoking certain new needs (subject to the qualifications previously mentioned), which has the effect primarily of *directing* inventive interest in certain channels rather than of accelerating the rate of invention; and second, by inducing a higher estimation of inventive activity because of the greater economic value of invention among large populations. Another means through which the growing density of population is said to accelerate invention, namely, by increasing social interaction, does not necessarily arise from heightened density alone, and may hence be considered as a factor independent of the concentration of population.

A large population has a greater number of inventive minds than a smaller group (keeping ethnic factors constant) and it is the absolute number of such inventors, rather than their proportion to the entire population, which is of primary significance for the rate of innovation. But, on the other hand, the mere number of "potential" inventors will not notably affect the rate of invention unless there is free communication between the inventors, a system of cultural values which places a high estimation upon innovation and an accumulation of knowledge which is at the ready disposal of the would-be inventors. The

last two conditions are cultural and civilizational, respectively, and need not concern us here. But the first of these factors, the kind and extent of social interaction, is perhaps the most important *social* element in the rate of invention.

A high degree of social interaction involves a number of processes which facilitate cultural change generally, and development in science specifically. The direct interaction which is afforded by face-to-face contacts is no more important than the circulation of sentiments, opinions, theories and facts which is made possible by various means of communication. Thus, both travel and communication stimulate and foment cultural change. Conversely, the relative lack of interaction is associated with what Teggart calls "the processes which are manifested in fixity, persistence, stagnation and conventionality." In such instances there is an absence of those intrusive factors which disrupt crystallized ideas and invoke all manner of change. The escape from traditionalism and the willing acceptance of the new is closely related to the rate, number and intensity of contacts.

Obviously, the contact of mind with mind (within certain cultural contexts) tends to stimulate observation and originality. Ideas and experiences which would otherwise have remained strictly personal may, through the medium of interaction, became elements of innovation and discovery. Observations may be made by one scientist for which he has no explanation, and were these observations not communicated to other investigators they would then have no significance for scientific development. But once they are submitted to others for explanation, once there is social interaction, there is a possibility (which is more probable, the more minds there are in contact) that these observations can be unified and systematized by a theory.

Thus, Jean Richer went from Paris to Cayenne (French Guiana) in 1671 to make astronomical observations for the purpose of finding the longitude. He found that his pendulum clock which had kept correct time in Paris fell daily two and a half minutes behind mean solar time. The pendulum was shortened, but upon his return to Paris, Richer found that it was too short. He could not account for this phenomenon, but upon its being communicated to Huyghens, this brilliant theorist ascribed the phenomenon partially to the greater centrifugal tendency of the earth in Cayenne. Thus, Richer's observation, which, had it not been communicated to others, would have had no bearing for the contemporary scientific advance, led to the theory that the same body receives different accelerations by gravity at different places on the earth.[10]

Incidentally, this same experience furnishes an illustration of the complex interlocking of the sociological factors which affect science. It is true, as has just been indicated, that the explanation of this phenomenon was found as a result of interaction between Richer and Huyghens; an explanation worked out in detail in the latter's "Horologium Oscillatorium." But one may push the sociological analysis further and ask how it happened that the original observation could have been made by Richer, and independently by Halley. The answer to this query indicates the intervention of other social elements. The finding of the longitude was an aim constantly stimulated by the needs of navigation and ultimately of commerce. This led to numerous expeditions being financed by various governments for the

[10] Maximilien Marie, "Histoire des Sciences mathématiques et physiques," Vol. V, pp. 102–5. Paris, 1884.

determination of the longitudes of important places throughout the world. It was upon such expeditions that both Richer and Halley made their observations of the shortening of the pendulum near the equator. It thus becomes evident how, in an admittedly roundabout and complex fashion, social and cultural factors converged to make possible Huyghens's abstract theory of the differential acceleration by gravity according to position on the earth.

Perhaps one of the most noteworthy examples in which interaction led to the introduction into the stream of scientific development of ideas which otherwise might have remained private involves Newton's "Principia." As is well known, the greater part of Newton's thoughts on gravitation was completed in 1666. For various conjectural reasons, the most probable being that he had not yet been able to determine that the attraction of a spherical body was concentrated at its center,[11] Newton put his work aside. It was only after his and Dr. Donne's visit to Wren in 1677, and Hooke's letter of 24 November 1679 asking Newton to comment on Hooke's theory of celestial motions, that Newton temporarily returned to problems of gravitation.[12] And it was only that "Newton was stimulated by Halley's visit of 1684 to return to the whole question of gravity . . ." which resulted in the actual publication of the "Principia."

There are a number of other ways in which social interaction influences the development of science. The laws used in science are selected from a number of possible laws which adequately state uniformities between the observed facts. Hence, the selection can not be made solely upon the basis of sheer correspondence to the facts. In other words, in the selection of a law which is stated to be "true," its truth (correspondence to the facts) is a necessary, but not sufficient, condition of its selection. The law is ultimately found acceptable because it fits into a theoretical structure the form of which is determined by preconceived ideas of what a theory should be.[13] Thus, in the seventeenth century, both the undulatory and corpuscular theories of light adequately accounted for certain phenomena of light, but the authority of Newton's apparent rejection of the wave theory led to its abandonment for the next century. Or to take a modern instance, Einstein and Eddington have severally presented a development of the theory of relativity for the unification of gravitational and electrical phenomena which correspond to the facts equally well. In short, from among the various theories which satisfy the observations the one chosen is selected because of the intellectual satisfaction which it affords. But the criteria of what constitutes intellectual satisfaction in any given period arise largely from the cultural scheme of orientation.[14] Thus, scientific laws are seen to have two primary aspects: the first, which is congruence with observed facts, may be called, following Campbell, the objective "truth" of the law; and the second, the fact that it provides intellectual satisfaction, may be termed its "meaning."

But scientific truth, in this restricted sense, is a quality which signifies the same for others as for ourselves and of which, moreover, the scientist can only be privately, and hence, not scientifically,

[11] *Cf.* Florian Cajori, "Newton's twenty years' delay in announcing the law of gravitation," in "Sir Isaac Newton," pp. 127–91. Baltimore, 1928.

[12] W. W. R. Ball, "An Essay on Newton's Principia," pp. 140, 155. London, 1893.

[13] N. R. Campbell, "Physics, the Elements," p. 157. Cambridge, 1920.

[14] Erwin Schroedinger, "Science and the Human Temperament," pp. 96 *ff.* New York, 1935.

certain until it has met with acceptance by other qualified persons. It is this fact which is basic to Lancelot Hogben's publicist view-point. For science is public, and not private, knowledge; and although the idea of "other persons" is not employed explicitly in science, it is always implicit. In order to "prove" a theory, which for the individual scientist on the basis of his own private experience may have already became a "true law" which requires no further confirmation, the investigator is compelled to set up critical experiments which will satisfy the other scientists engaged in the same cooperative activity. This pressure for so working out a problem that the solution will satisfy not only the scientist's personal criteria of validity or adequacy, but also the criteria of the group of scientists with whom the investigator is actually or symbolically in contact, constitutes a powerful social impetus for cogent, rigorous scientific development. The work of the scientist is at every point influenced not only by the intrinsic requirements of the phenomena with which he is dealing, but more directly by his reacting to the inferred critical attitudes or actual criticism of other scientists and by an adjustment of his behavior in accordance with these attitudes.

Thus, Fahie cites Galileo as having written that "ignorance had been the best teacher he ever had, since in order to be able to demonstrate to his opponents the truth of his conclusions, he had been forced to prove them by a variety of experiments, though to satisfy his own mind alone he had never felt it necessary to make any."[15] Or, to take another instance, Boyle would probably never have discovered the law bearing his name were it not for the criticism of his "New Experiments . . . touching the Spring of the Air" made by Franciscus Linus. Linus, in examining Boyle's work, declared that the air is definitely insufficient to achieve such great effects as counterpoising a mercurial cylinder of twenty-nine inches. Linus maintained that the mercury hangs by invisible threads (funiculi) from the upper end of the tube. This criticism incited Boyle to renewed research, which finally led to the experimental validation of the theory "that supposes the pressures and expansions of gases to be in reciprocal proportion."[16]

The history of the establishment of this law also illustrates another consequence of social interaction which we have previously discussed; namely, the provision of new contexts which render significant previously meaningless observations. Hooke had in 1660 made similar experiments upon the rarefaction of the air, but he did not organize these results into any coherent theory. It was only in the following year, when he heard of Boyle's hypothesis, that he repeated these experiments over a greater range with results substantially verifying the theory. In other words, the meaningfulness of mere observation resulted from a hypothesis derived from contact with contemporary scientists.

The rôle of criticism is further illustrated by Hooke's animadversions upon Newton's theory of light, which resulted in the latter submitting a large number of considerations which he had not raised in his initial papers. Newton was compelled to institute a number of additional experiments to test aspects of his theory which he had not previously considered.

[15] J. J. Fahie, "The Scientific Works of Galileo," in "Studies in the History and Method of Science" (ed. by Charles Singer), Vol. II, p. 251. Oxford, 1921.

[16] Robert Boyle, "New Experiments Physico-Mechanicall, touching the Spring of the Air, whereunto is added A Defence of the Author's Explication of Franciscus Linus and Thomas Hobbes," p. 100. Oxford, 1662. The title of this book indicates the effectiveness of criticism in evoking continued and more rigorously executed research. *Cf.* Florian Cajori, "A History of Physics," p. 73. New York, 1924.

In this sense is conflict the "gadfly of thought."

A telling indication of this process is found in the fact that scientific theories and laws are presented in a rigorously logical and "scientific" fashion (in accordance with the rules of evidence current at the time) and *not* in the order in which the theory or law was derived. This is to say, long after the theory has been found acceptable by the individual scientist on the basis of his *private* experience he must continue to devise a "proof" or "demonstration" in terms of the approved canons of scientific verification present in his culture. As Poincaré has indicated, most important scientific discoveries have been divined before they have been demonstrated. But intuition, howsoever powerful an instrument of invention it may be, is never a sufficient basis for a doctrine to become incorporated as a part of science. "Demonstration" is still necessary.

Jean Piaget has described the differences between one's private way of developing his thoughts and the order in which they are presented to others.[17] The logical and empirical ramifications of a doctrine must be worked out before it is accepted by others; implications must be pursued far beyond the point where an individual, not in contact with similarly equipped critics, might be content to rest his analysis. In this way, social interaction provides a definite incentive for highly rigorous scientific investigation. As Piaget has observed, children gradually learn to socialize their beliefs by logical and factual proof only as a result of conflicts of opinion between them. When simple affirmations or denials are seen, through a series of disconcerting experiences, to possess no cogency, "reasons" are gradually submitted to justify their beliefs. On a higher plane, as is evidenced in the previously cited illustrations, the same process is found in scientific development. The greater the number of critical minds surveying the evidence the more exacting is the compulsion to work out the ramifications of a theory so that it will become scientifically acceptable.

To summarize, one may say that a high degree of social interaction tends to foment rebellion against traditionalism and to make for the willing acceptance of novelties and innovations. It tends further to enhance scientific development by increasing the probability that scattered observations will be unified by some generalizing theory. Finally, criticism (as one form of social interaction) increases the pressure upon scientists for methodically following through the implications of their theories beyond the point where they may have felt personally content with their research. In this way, the rate of scientific development is materially increased and the resulting theoretic structure maintains a high degree of inner consistency and coherence.

THE INTERNATIONAL MISSION OF SCIENCE

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If there is anything objective in human nature, it must be science; if there is anything in this world that does not stop at national or military frontiers, it is the scientific thirst for the conquest of the unknown, the scientific curiosity to look behind the stage of life and to solve the mystery of the cosmos.

[17] Jean Piaget, "Judgment and Reasoning in the Child," Chaps. V, IX. London, 1929.

This is, of course, an assertion, and we shall have to prove it. Let us consider, then, some of the reasons why science should be objective. First of all, the subject of science is independent of any individual or nation. If it is found by an Englishman that light and radio waves are essentially the same phenomena, any one on earth might repeat the crucial experiments and will obtain the same results under similar conditions. Let me emphasize the same result. Where in any other occupation of the mind, be it religion, philosophy, law, economics or even the social sciences, can this be said? But more, the most standardized language on earth is mathematics, and just mathematics is the universal means of expressing scientific truths. Be it in the form of a theory or in the form of quantitative experiments, the mathematical relations form the backbone of true science. In fact, it is even presumed at times that nothing should be called science unless it could be brought into the form of mathematical relations. This may, of course, be going too far. Nevertheless, it indicates that the method of scientific investigation is the same in all countries and for all individuals. A third reason, and the final one I shall consider here, is the fact that scientific knowledge has grown to such vast proportions that it is humanly impossible for any one man to master all of it. Any scientist interested in a particular subject, who sets out to do research, must inevitably study first what has been done in this field. Without this information, he might spend a lifetime trying to reestablish discarded theories or to rediscover what others had found long before him. It is necessary for a scientist to study not only the local, but the world literature in order to keep abreast of the events. But this means continued contact with foreign publications, as well as with the national ones. It means cosmopolitan interests as against national interests. And it means, most important of all, admiration for the progress of scientific thought and discoveries in other countries and, connected with it, a deep admiration for the achievements of the human mind.

Thus we find three phases in scientific endeavor which tend to make science truly objective: the subject itself, the method of pursuit and the study of progress of science. But—you could raise these questions—are not scientists to be thought of as individuals of a particular nation, bound by tradition and inheritance to be prejudiced in favor of their own nation? Will they, in case of war, not be acclaiming their own nation and denouncing the enemy nation? And I must answer: Yes, scientists fought in the last world war under all flags, and probably will do so again in a future war. As individuals, they can not escape the national bonds, the bonds of the larger family they happen to be born in. Scientists are not superhuman beings, but they are the qualified missionaries for international understanding which is progressing, as I optimistically might say, despite recent events. The continued occupation with science of one group of individuals in every nation can not but influence and in this way broaden the point of view of its other groups in the direction of international good will. The immense fertilization of objective ideas will eventually lead to a true League of Nations based upon mutual respect rather than mutual distrust.

Let me give you some reasons for my optimism by illustrating the international cooperation of science toward a common goal. The tremendous upswing in scientific development started in 1820 with the accidental discovery by the Danish scientist Oersted that an electric current will deviate a magnetic needle into a definite direction. Up to that time no one had ever thought of electricity or magnetism as other than entirely unre-

lated playthings, to amuse people. The new experiments, however, connected electricity with magnetism. About fifty years of research, in which the scientists of all countries participated, brought electricity into the household, made it indispensable in factories and introduced names familiar to every one, such as the Frenchman Ampere, the Italian Volta, the Englishman Faraday, the German Ohm, the American Henry, and others.

But science did not stop there. Once it had given the world the basis for undreamed-of engineering projects, it moved on into the deeper jungles of electromagnetic phenomena. Again, scientists of all countries attacked at once the phenomenon of light, and discovered in less than forty years what Sir William Bragg so aptly called the "universe of light." For a short time it had been thought possible to explain light as a particular case of radio waves of extremely short wave-lengths. But to-day it seems more reasonable to assume that all radiation is of the same type as light, that radio waves, heat radiation, ultra-violet, infra-red and x-rays, even the cosmic rays, are all but different aspects of the same fundamental phenomena. Unity displayed in the amazing diversity! Simplicity hidden behind the confusing variety of radiations with so many different effects!

Perhaps this should serve us as a warning. Perhaps this should illustrate to us some law of nature in producing many human races and nations instead of a single standardized international human being. The "universe of light" might be comparable to humanity, and the various kinds of radiation to the different nations. Without the variety of radiations, indeed, life could not exist on earth in its present form. Without the different characteristics of the kaleidoscopic nations we probably could not succeed so quickly and so well in advancing our scientific knowledge, because—and this is a fundamental truth—mankind needs stimulation, and variety of view-points, in order to incite those flashes of genius that mark the large steps forward. In exploring the nature of radiation, the Germans Planck and Schroedinger, the Englishmen Maxwell and Rutherford, the Dane Bohr, the Frenchman de Broglie, the Americans A. H. Compton and Millikan, to name just a few of the most outstanding ones, had to contribute in succession and alternately in order to build the "universe of light."

The rapid development of scientific knowledge, which we witness to-day, is not only due to better methods, but has also been the result of an accelerating pace because of the increasing number of scientifically trained people. Thus, as time goes on, larger groups within the nations take part and are interested in scientific research, transcending in this manner the national bonds at least temporarily, and gaining respect and appreciation for the achievements of other nations.

It is obvious, then, that science has a true international mission. Far from choosing propagandistic methods to promote internationalism, science creates the true atmosphere of humanity as a common bond of the many diversified races and nations, just as the universe of radiation is the super unit of light and heat, of death rays and life rays. As it is important to recognize the facts and advantages of racial and national differences, so it is equally important to consider nationalism only as a first step towards true international understanding, based upon respect and appreciation for the services of each individual nation to humanity.

INSECT TRANSMISSION OF VIRUS DISEASES OF PLANTS

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No group of plant diseases has attracted as much attention in recent years as those caused by viruses. A half century ago we did not recognize a half dozen distinct diseases which are now attributed to viruses. We did not know their causes and very little about their character. To-day we know that more than a thousand species and varieties of plants are subject to these diseases. We call them virus diseases, but we do not know with certainty the character of the active agents causing them, though we do know some of the properties of these active agents. We do not know with any degree of accuracy the number of distinct so-called viruses or active agents causing these diseases in this long list of host plants, but we do know that they are the causes of heavy losses in our agricultural crops.

All students of the subject recognize the importance of knowing more about the causal agents of these diseases. One of the most important phases of the subject is a knowledge of the methods of transmission of the causal agents from plant to plant and from locality to locality in nature if we are to control them and reduce these heavy losses to the minimum. The early students of these diseases were not slow to find that the causal agents could be transmitted by inoculation with juices and by budding and grafting. Some years later it was learned that some of them are transmitted by cuttings, tubers and bulbs from diseased plants. The early students announced that the diseases studied by them were not transmitted by seeds from diseased plants, but later studies have shown that a considerable number of them are transmitted in this manner, although in some cases the percentage is extremely small.

Probably the most important discovery was that some of them were transmitted by insects. This discovery was not made in a day. In fact, there was an interval of fifteen or twenty years from the time when insects were first recognized as probable carriers until the importance of this method of transmission was demonstrated. Since that time our knowledge of this method of transmission has increased very rapidly and we now recognize more than 135 species of insects as vectors of these diseases.

Smee of England (1846) reported that *Aphis vastator* was very abundant on potatoes with "curl," which was probably a virus disease, but this announcement did not attract any attention. Woods (1897) called attention to the presence of aphids associated with the Bermuda lily disease, which is now known to be due to a virus, but he believed that the insects caused a starvation of the plants and that the disease was due to an increase in the amount of enzymes.

It is interesting to note that the two discoveries which led to our present knowledge of the relationship of insects to virus diseases were made before our present ideas of the causes of these diseases had been developed. The first students of this phase of the subject believed that the insects were the causes of the diseases.

The first records that are at all definite in regard to the transmission of virus diseases by insects that have come to the attention of the writer are those concerning the "dwarf of rice" in Japan and the

"curly top of sugar beets" in the United States.

It appears that Hashimoto (1894–95), a Japanese farmer, was the first to prove experimentally the relationship of leafhoppers to the dwarf disease of the rice plant, but he did not publish any of his data.

Takata (1895–96) was the first to write on the causal relationship of the leafhoppers. He stated that the leafhopper was known as "mon-yokobai," which was probably *Deltocephalus dorsalis* Motsch, but his determination of the insect was evidently an error. These two men should have the credit of this discovery, although they did not understand the cause of the disease and the determination of the vector was incorrect.

A brief note in the *Official Gazette* of the Japanese Government (1890) states that the leafhoppers were abundant on the rice plants in localities where the disease was prevalent.

Report I (1897) of the Shiga Agricultural Experiment Station mentions several leafhoppers as causing the dwarf disease of the rice plant, but Report II (1900) states that only one species, *Nephotettix apicalis* Motsch var. *cincticeps* was the true cause.

Takama[1] summarized the work of this experiment station and concluded that the disease was due to this insect. It was later (1906) demonstrated that the insects from some parts of Japan would produce the disease, while those from other parts would not do so. This was confirmed two years later. Still later it was proved that all of them would produce the disease if fed on diseased plants and then transferred to healthy plants.

The Imperial Agricultural Experiment Station at Tokyo was the first to prove (1906) that *Nephotettix apicalis* var. *cincticeps* did not cause the disease but was the carrier of the active agent that caused it. The Shiga Agricultural Experiment Station confirmed this work and was the first to report it (1908). The writer has not been able to learn the names of the persons who demonstrated the proof and made the report.

The Japanese name of the insect is "tsumagoro-yokobai," and it was described in 1896 by Uhler under the name *Seleocephalus cincticeps*.[2] Matsumura considered it a variety of *Nephotettix apicalis* which he had previously established.[3] It was originally described in India by Motsch as *Pediopsis apicalis* in 1859.[4]

The first virus disease of plants in America that was proved to be carried by an insect was curly top of the sugar beets. This disease was reported on sugar beets in California about 1899 and came to be so injurious to the crop that it attracted a great deal of attention and was the subject of study by several American scientists. It is very evident that the disease came from wild plants, which are now known to be susceptible. The history of our knowledge of the transmission of this disease by an insect is briefly as follows.

Townsend (1902) described the symptoms of the disease. R. E. Smith (1906) stated that it was a definite disease and the result of unfavorable environmental conditions. Ball (1906) called attention to the large number of *Eutettix tenella* Baker and said that its punctures "seemed to cause a sort of thickening of the veins of the leaf and an unhealthy condition called curly leaf or blight."

Ball (1907) suggested that the disease might be similar to galls of plants which are caused by insects.

Townsend (1908) called attention to the presence of *E. tenellus* on growing sugar beets and said that the growers believed these insects to be the cause of the curly top disease. He also called attention to the insect feeding on *Rumex crispus*. The insect is now known to feed

[1] *Jour. Agr. Soc.*, 241: 22–30, 1901.

[2] Proc. U. S. National Museum 1896, p. 292.

[3] Trans. Sapporo Nat. Hist. Soc. 1905, p. 20.

[4] Etude Ent. 1859, p. 110.

on several species of wild plants which have been proved to be susceptible to the curly top disease.

Ball (1909) published the results of observations and experiments made in 1905 and 1906. He said:

> As a result of the season's observations there seems to be little question that the "curly leaf" condition was result of the attack of the leafhopper combined with the effect of a very hot, early season. . . .
>
> In the case of the "curly leaf," however, the abnormal condition apparently spreads from leaf to leaf until finally the whole plant is affected, even though the leafhoppers may have disappeared before the process is complete.

Ball (1909) reported cage experiments for the study of the life history of the insect. The beet plants in two of these cages died in a short time.

Adams (1909) records the putting of four *E. tenella* in a cage with a healthy beet plant, which developed the disease later.

Spisar (1910) expressed the opinion that the disease was not caused by the punctures of the insect, but agreed with Ball, who had previously expressed the opinion that the abnormalities were due to something which the insect injected into the plant.

Shaw (1910) reported the result of cage experiments in which he found that the leafhoppers from the western regions in which the disease occurred could produce the disease in sugar beets grown in cages in Washington, D. C., where the disease did not occur. He believed that the insects were the cause of the disease and that it did not exist outside the range of the insect. He did not know the cause of the disease, but suggested that it might be due to some active agents injected into the plant by the insect.

The results of Ball, Adams and Shaw in their cage experiments were confirmed by R. E. Smith and Boncquet (1915), who demonstrated that a single insect from a diseased plant placed on a healthy plant for five minutes would result in the development of the disease. They concluded that it was possible for the insect to extract the active agent from a diseased plant in three hours, but that 24 to 48 hours must elapse before the insect could transmit this agent into a healthy beet plant. This work indicated that the disease was not due to insect injuries, but they did not say anything about a virus. A little later in the year, Boncquet and Hartung reported that the insects collected from *Artemisia* or *Atroplex* of the upper Joaquin valley did not produce the disease, while those from diseased beets at Spreckels, California, did transmit the disease to healthy plants. They also demonstrated that when these insects from healthy plants were transferred to diseased beets for a time and then returned to healthy plants, the disease appeared in due time. This was a very definite demonstration that the leafhoppers were the carriers of the active agent of the disease.

Up to this time our ideas as to the cause of this disease were very indefinite, as shown by the following statement in a paper by Boncquet and Stahl (1917):

> The curly-top condition of sugar beets has for some time been a subject of investigation by phytopathologists, but on account of the failure to discover a specific organism responsible for the physiological injury to the plant, the problem has been peculiarly baffling. Although the connection of the jassid leafhopper, *Eutettix tenella* Baker, with the disturbance has been definitely established, conclusive evidence has not been available as to the exact nature of the trouble, or as to the part played by the leafhopper in the dissemination of the virus.

Ball (1917) published a bulletin in which he reviewed our knowledge of the entire subject. Some of his comments which are quoted below show the extent of our knowledge of the subject at that time. He said:

> There are a large number of plant diseases for which no causal organism has as yet been found, curly leaf among the number. . . .
>
> A puncture of the beet leafhopper is absolutely necessary to cause the disease to develop

in the beet. Under favorable conditions Titus found that a very short application of a single insect would produce the disease on a young beet. . . .

The disease never spreads from beet to beet except as it is transmitted by the leafhopper. . . .

The punctures of the beet leafhopper (*Eutettix tenella* Baker) cause a specific disease of the sugar beets called "curly leaf." . . .

Curly leaf has never been produced except through the punctures of a beet leafhopper.

Within the last fifteen or twenty years it has been demonstrated that the virus of this disease will attack tomatoes, beans, squashes and many wild plants. The insects feed on these hosts and transmit the disease from species to species. It appears that the insects pass the winter on some of the susceptible wild host plants and travel from them to the young crop of sugar beets, carrying the active agent which causes the disease.

A study of the literature on these two diseases indicates that the early workers believed that the insects were the causes. However, the work of Takata and Ball demonstrated the importance of the study of insects in connection with this great group of diseases which are now known as virus diseases.

Clinton and Allard called attention to the presence of insects on diseased plants, and in 1916 Doolittle and Jagger, working independently, demonstrated that *Aphis gossypii* was a carrier of the cucumber mosaic. From that time to the present studies of this kind have been very fruitful and it has been found that about 150 species of insects are carriers of virus diseases. Some of them carry but one virus so far as known, while others carry several viruses. Records up to the present time indicate that *Myzus persicae* is capable of carrying more viruses than any other species.

These studies have been expanded and many interesting facts brought to light which can not receive more than a brief mention at this time.

It is well known that some insects are mechanical carriers; *i.e.*, the virus is smeared on the mouth parts while feeding on diseased plants and then rubbed off while feeding on a healthy plant. In most cases the virus is sucked up into the insect and then injected into the healthy plant. It is not known whether the virus undergoes a change in the insect or not, but there is evidence that it does increase in amount.

Stahl and Carsner (1918) and Severin (1922) reported that *E. tenellus* did not transmit curly top virus to its offspring. McClintock and Smith (1918) reported the transmission of the spinach blight through four generations of *Myzus persicae* and *Macrosiphum gei*, but Hoggan (1930) failed to confirm these results with cucumber mosaic, which she found to be one of the three viruses causing the spinach blight. It is possible that McClintock and Smith used one of the other two viruses. Fukushi (1933) reported the transmission of the virus of dwarf disease of rice from parent to offspring through the egg of *Nephotettix apicalis* var. *cincticeps*. This is the first definite record of this kind.

Kenneth M. Smith, of England (1928), discovered that when a potato mosaic with which he was working was transmitted by needle punctures to a healthy tobacco plant the symptoms were different from those that developed when the transmission was made by means of *Myzus persicae*. It was found that this mosaic was the result of a mixture of two viruses, both of which were transmitted by the needle inoculations and only one by the insect inoculations. Hoggan (1930) reported that *Myzus pseudosolani* and *Macrosiphum solanifolii* (*M. gei*) were able to transmit tobacco mosaic virus from tomato to many solanaceous plants but not from tobacco; that *Myzus circumflexus* was able to transmit the virus from tobacco. These studies indicate that some viruses may be isolated by the insect vectors and that this may be an important factor in the classification of viruses.

CHANGES IN NERVE FIBERS DURING ALCOHOLIC INTOXICATION AND RECOVERY[1]

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THROUGHOUT the ages alcoholic drinks have been popular with mankind. Recent events in this country strongly suggest that this popularity is not on the wane.

Alcoholic intoxication is accompanied by certain definite symptoms familiar to every one. The sensory mechanism, the motor mechanism, the coordinating mechanism and the mental processes are all affected. All these mechanisms are known to be intimately related to the proper functioning of nerve cells. It is beyond doubt, therefore, that alcohol must act upon the nervous system.

The *functional* disturbances associated with alcoholism have been rather thoroughly studied. The *structural* disturbances, however, have never been satisfactorily investigated. Martland[2] tersely sums up the subject with the statement that "extensive permanent injury to the cells of the brain, spinal cord and nerves is, on the whole, unusual or at least does not appear on a gross and microscopic study of the tissues after death." He, nevertheless, considers that alcohol produces its main damage in the nervous system.

For several years, I have been watching the behavior of nerves in living organisms by direct observation. Small frog tadpoles are used. An animal is slightly anesthetized and placed on a specially prepared microscopic slide. Individual nerve fibers are then observed in the transparent tail fin under very high magnification. A map of the nerves is made, the animal replaced in pond water and on the next day the same region and the same nerves are again studied. By this method histories of individual nerve fibers over a period of several months may be obtained. Thus the growth of nerve sprouts, the formation of the nerve sheaths and the changes during nerve injury and repair have all been seen in the living tadpole.[3]

It occurred to me that it might be interesting to watch nerve fibers while the tadpole was subjected to prolonged and repeated intoxication by alcohol. This proved to be entirely feasible. My records now include many case histories of nerve fibers which demonstrate that practically all graduations of irritation and injury may be induced by alcohol treatment. Illustrative motion pictures showing such irritated nerve fibers have also been made directly from the living animals.

Before describing a few of these cases, the following points may be noted. Poisoning of nerve cells by alcohol depends upon the concentration of alcohol in the blood and body fluids. It matters little as to the mode of entrance. In the tadpole, the alcohol is administered by way of the skin. The moist skin of the frog tadpole is readily permeable to alcohol, being comparable in this respect to the lining of the alimentary tube of man.

Fortunately, the nerves of the frog

[1] Complete details of this study have been published in the *Journal of Comparative Neurology*, Vol. 64, under the title "Studies of Living Nerves. V. Alcoholic Neuritis and Recovery."

[2] In chapter IX of "Alcohol and Man," edited by H. Emerson.

[3] Details are published in the *Journal of Experimental Zoology*, Vol. 61; *American Journal of Anatomy*, Vol. 52; *Journal of Comparative Neurology*, Vol. 61, and the *Biological Bulletin*, Vol. 68.

tadpole are constructed essentially on the same plan as those of man. Single mature fibers of the type which bring in sensation from the skin are readily available for observation. The chief parts of such a fiber are shown in the diagram (Fig. 1, a). A conspicuous feature is the myelin sheath, a fatty covering which encases the nerve axis and protects, insulates and nourishes it. This sheath is in the form of segments separated by constricted nodes, the arrangement resembling a string of sausages. As it responds quickly to irritation of almost any sort, it is an excellent indicator of alcohol effects. It may swell greatly, degenerate or partially degenerate, even though the nerve axis cylinder within remains alive (Fig. 1, b, c, d). It never persists, however, if the axis cylinder degenerates.

Outside the myelin sheath near the middle of each segment lies the sheath cell nucleus. A very delicate membrane (called the neurilemma), which is a part of the sheath cell, forms a tubular covering for the nerve fiber. It is not in segments like the myelin sheath, but extends from segment to segment over the nodes as a continuous membrane. It is best seen as the myelin sheath fragments (Fig. 1, d).

Answers were sought to the following questions:

(1) Exactly what structural modifications take place in nerves during alcoholic intoxication?

(2) What are the steps of recovery after both mild and severe cases of intoxication?

(3) Can complete degeneration of myelin sheath segments be induced?

(4) Does repeated alcoholic intoxication stop the addition of new myelin sheath segments in a growing zone?

(5) What are the relative effects of a single prolonged continuous period of strong intoxication (the "spree" type) and of short daily periods of mild intoxication continued, however, for many weeks?

(6) Does repeated alcoholic intoxication stop growth of nerve sprouts?

(7) How do nerve endings react during various degrees of alcoholic intoxication and recovery?

Effects of alcohol on myelinated nerve fibers: In very dilute alcohol (less than 0.5 per cent.) tadpoles may live indefinitely with little or no indication of any special nerve irritation. In much stronger alcohol solutions (more than 3 per cent.) death usually ensues within a few hours. The skin suffers direct injury in these.

Alcohol solutions in the neighborhood of 2 per cent. are very effective in bringing on marked nerve changes.[4] A strongly irritated nerve fiber exhibits the following changes (Fig. 1, b, c, d): marked swelling, undulating movements of the myelin sheath, appearance of watery spaces between the myelin sheath and the enclosed nerve axis, followed by gradual separation of these structures, assumption of an irregular wavy course by the nerve axis, and in extreme cases, degeneration of the myelin segment with the formation of myelin globules and ovoids. Similar changes in nerve fibers take place in tadpoles treated with wood alcohol.

Recovery quickly follows the milder treatments with alcohol. Irritated fibers which reach the stage shown in Fig. 1, b, usually recover completely in one or two days. A few small myelin globules may be pinched off from the sheath.

A fiber which reaches the greatly swollen stage shown in Fig. 1, c, usually recovers completely in two to five days. During the recovery the swelling subsides as fluid is expelled. Several myelin ovoids may be formed, each of which

[4] The effect of a given percentage of alcohol varies somewhat, depending on the age, size and species of tadpole. Older animals are less susceptible.

later may break up into smaller globules. Such a recovering fiber is shown in Fig. 1, e. Occasionally, however, a swollen myelin segment (such as that of Fig. 1, c) fails to recover and suffers subsequent degeneration (Fig. 3, segments R and S). In such cases a completely new segment may be regenerated. New myelin is formed through the cooperation of the sheath cell and the nerve fiber.

Myelin segments which reach the beaded stage of irritation shown in Fig. 1, d, usually degenerate completely or partially.

A number of tadpoles have been watched to see how nerves react to alcoholic intoxication of brief duration repeated each day for many days. Even strong doses which cause visible irritation do not necessarily cause permanent injury. Ready recovery may take place. In some instances the older segments of the sheath may become somewhat creased and wrinkled, a condition which probably indicates slight chronic irritation.

One interesting case may be cited in detail (Fig. 2). A tadpole became strongly intoxicated after living in 2 per cent. alcohol for 21 hours. Two of the last myelin sheath segments of a nerve fiber started to degenerate. On replacement of the animal in pond water, however, these segments recovered in a few days. Two more strong treatments were given at 2-day intervals. Thereafter, for a period of 75 days, the animal received enough alcohol once a day to stun it slightly. Temporary irritation of the fiber was usually visible during these treatments. However, the fiber readily recovered each day between treatments and remained fairly normal in appearance. Moreover, the youngest myelin segments grew some in both length and thickness. Some new nerve sprouts were also formed and three new segments were added near the end of the fiber. This case and others of similar nature suggest that daily intoxication of brief duration is not enough to cause degeneration of either myelin sheath or nerve axis. Nor is it sufficient to prevent the growth of nerves in regenerating zones.

On the other hand, a single severe prolonged treatment may cause marked injury of nerves, especially toward the tips. In one case an animal was kept in 2.5 per cent. alcohol for five hours. At the end of this period it was dazed ("dead drunk," in other words) and could be examined without the use of any anesthetic. The speed of the circulating blood was markedly less than normal. A long nerve fiber was observed with its myelin sheath in process of degeneration (Fig. 3). The last 13 myelin segments (that is, those nearest the tip of the nerve) were already breaking up into fragments. Those farthest from the nerve tip were all greatly swollen and exhibited definite separation of the myelin sheath from the nerve axis. The animal was then taken from the alcohol solution and put back in pond water and the process of recovery watched. During the next two days, two more of the swollen myelin segments[5] broke up into fatty ovoids. The others slowly recovered. At many points on these recovering segments, however, small myelin globules were cut off, an indication of the strong irritation to which the nerve fiber had been subjected. During the next two months, new nerve sprouts grew from the injured fiber to make entirely new connections with the skin.

Many attempts were made to induce degeneration of all myelin segments along a fiber. These invariably failed.

[5] In man, the day after a "spree" is often characterized by a headache. In tadpoles 12 to 24 hours after a period of severe alcoholic intoxication, many myelinated fibers are swollen to twice their normal diameter. If such a condition also obtained in the brain and spinal cord there would probably be a certain amount of compression. The headache of "hangover" day thus might be directly ascribable to this characteristic swelling of myelinated fibers.

Death of the animal ensued before fragmentation of the more proximal myelin segments of the fiber. Probably the treatments caused injury to some part of the nervous system associated with control of a vital activity, such as the heart beat.

This property of nerve fibers to degenerate peripherally is of interest. In cases of human alcoholic neuritis, as well as in various other forms of multiple neuritis, there is a loss or impairment of nerve function first near the nerve tips. The sensory and motor functions of the limbs are particularly disturbed; also, though to a less degree, those of the arms. My observations on alcoholic nerve irritation in tadpoles agree with this and give direct visual evidence that degeneration starts near the nerve tips and progresses centrally. Thus there is close correlation between the anatomical effects and the clinical symptoms.

Effects of alcohol on the growing tips of regenerating nerve fibers: When a nerve fiber is cut it regenerates. The peripheral part degenerates and is lost. The central part develops at its tip a "growth cone," which progresses by a slow flowing motion through the tissues spinning a fiber behind it. These growing tips are easily affected by adverse conditions. When alcohol treatment is given, a growing tip may retract quickly. After normal conditions are restored the nerve tip may again within 10 or 15 minutes resume its growth. In the example given (Fig. 4) growth, retraction and recovery are all shown. In this case and in other cases, about a half hour's treatment with alcohol was sufficient to cause a regenerating nerve tip to retract a distance of about one thousandth of an inch. The rate of extension of the nerve tip during recovery was somewhat less. About an hour was often required for growth equal to one thousandth of an inch. Motion pictures of the "fast motion" type reveal in dramatic fashion the

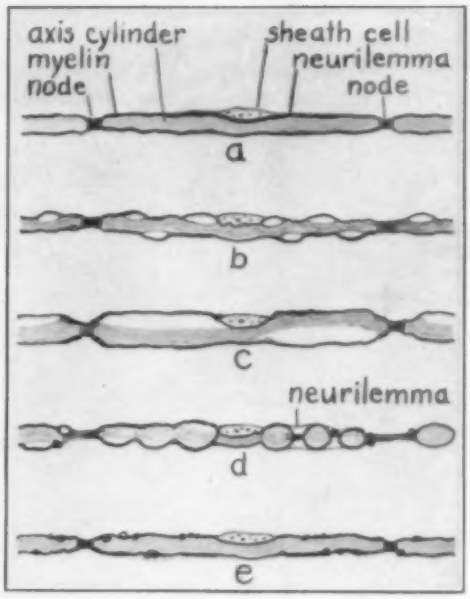


FIG. 1. CHANGES IN A NERVE FIBER OF A FROG TADPOLE DURING ALCOHOLIC INTOXICATION AND RECOVERY.

a, NORMAL APPEARANCE OF A SINGLE NERVE FIBER SHOWING THE POSITION OF THE AXIS CYLINDER, MYELIN SHEATH, SHEATH CELL, NEURILEMMA AND NODE BETWEEN THE MYELIN SEGMENTS. *b*, DURING EARLY STAGES OF MODERATE IRRITATION WATERY SPACES APPEAR BETWEEN THE MYELIN SHEATH AND AXIS CYLINDER. SMALL MYELIN GLOBULES MAY BE CUT OFF FROM THE SHEATH. *c*, A LATER AND GREATER DEGREE OF IRRITATION IS CHARACTERIZED BY MARKED SWELLING. *d*, EXTREME IRRITATION IS CHARACTERIZED BY FRAGMENTATION OF THE MYELIN INTO OVOIDS. THE DELICATE OUTER SHEATH (NEURILEMMA) IS OFTEN CLEARLY VISIBLE AT SUCH A STAGE. *e*, APPEARANCE OF A RECOVERING FIBER TWO DAYS AFTER STRONG IRRITATION. A FEW SMALL GRANULES OF MYELIN DEBRIS ARE THE ONLY INDICATION OF THE PREVIOUS IRRITATION.

retraction of nerve tips in alcoholized tadpoles and the subsequent growth during recovery.

These reactions are very conspicuous. They are important also because in somewhat exaggerated form they give a fairly good picture of what takes place sometimes at the nerve endings of resting nerve fibers.

Effect of alcohol on nerve endings: In

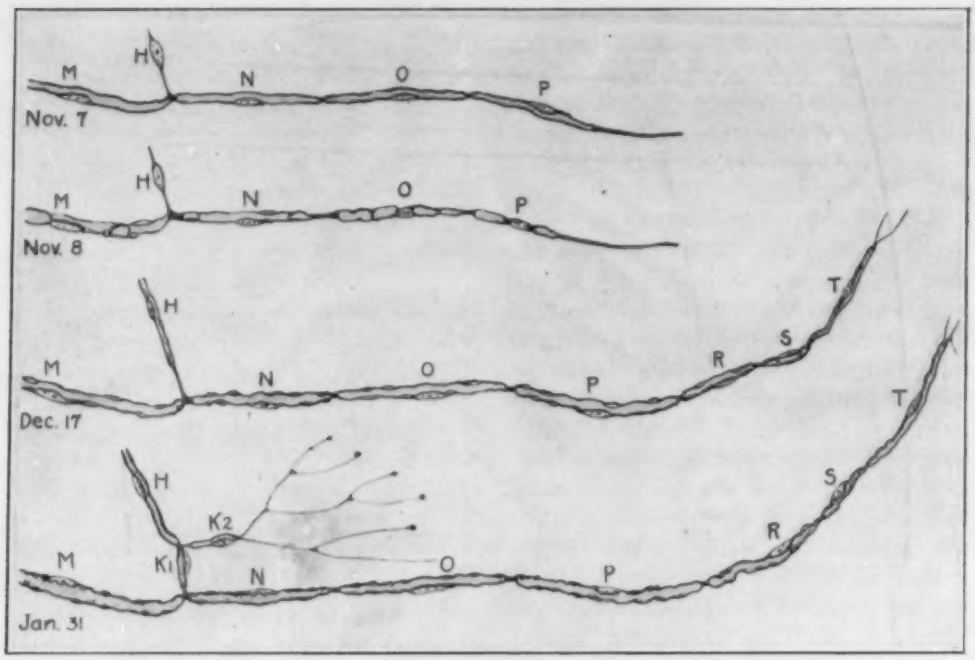


FIG. 2. EFFECTS OF DAILY ALCOHOL TREATMENT FOR A PERIOD OF NEARLY THREE MONTHS.

ON NOVEMBER 7 A TADPOLE WAS IMMERSED IN 2 PER CENT. ALCOHOL FOR 21 HOURS. A FIBER, OF NORMAL APPEARANCE BEFORE THE TREATMENT, BECAME MARKEDLY IRRITATED. ON NOVEMBER 8 WHEN THE ANIMAL WAS REPLACED IN WATER THE MYELIN SEGMENTS WERE SWOLLEN AND THE LAST TWO WERE IN PROCESS OF BREAKING UP. NEVERTHELESS, ALL THE MYELIN UNITS RECOVERED WITHIN A FEW DAYS. SIMILAR TREATMENTS WERE GIVEN ON NOVEMBER 14 AND NOVEMBER 16 WITH SIMILAR RESULTS. THEN FROM NOVEMBER 18 TO FEBRUARY 1 THE TADPOLE RECEIVED ENOUGH ALCOHOL ONCE A DAY TO STUN IT SLIGHTLY. EACH DAY DURING THE TREATMENT VISIBLE CHANGES OF IRRITATION OCCURRED (*i.e.*, VACUOLATION, SWELLING AND THE CUTTING OFF OF SMALL MYELIN GLOBULES). NEVERTHELESS, THE FIBER SLOWLY GREW AND NEW MYELIN SEGMENTS WERE ADDED. THREE OF THESE (SEGMENTS R, S AND T) WERE NEAR THE END OF THE FIBER; THREE (SEGMENTS K_1, K_2 AND H) WERE ALONG A COLLATERAL BRANCH.

the frog tadpole the ordinary sensory nerve endings in the skin are naked. As a myelinated fiber approaches its termination it loses its sheaths, *i.e.*, both the myelin sheath and the neurilemma. It branches freely and ends in about 15 to 30 tiny spherical or ovoid bulbs. These are located just beneath the basement membrane of the epidermis, which is the outer covering of the skin. On the whole, they are fairly stable structures. I have watched many of them for days or weeks during which no essential changes took place. Occasionally, however, a change of either growth or retraction may occur.

Careful observation reveals that decided effects may be discerned in these as a direct result of alcohol treatment. A typical case is illustrated (Fig. 5). With suitable strengths of alcohol there may be induced swelling, retraction and sometimes nerve autotomy[6] (*i.e.*, the self-

[6] If a lobster is held by one of its appendages, so that it is unable to get away, the appendage may be pinched off, thus freeing the lobster. This ability of an animal to separate itself from one of its limbs is called autotomy. Nerve

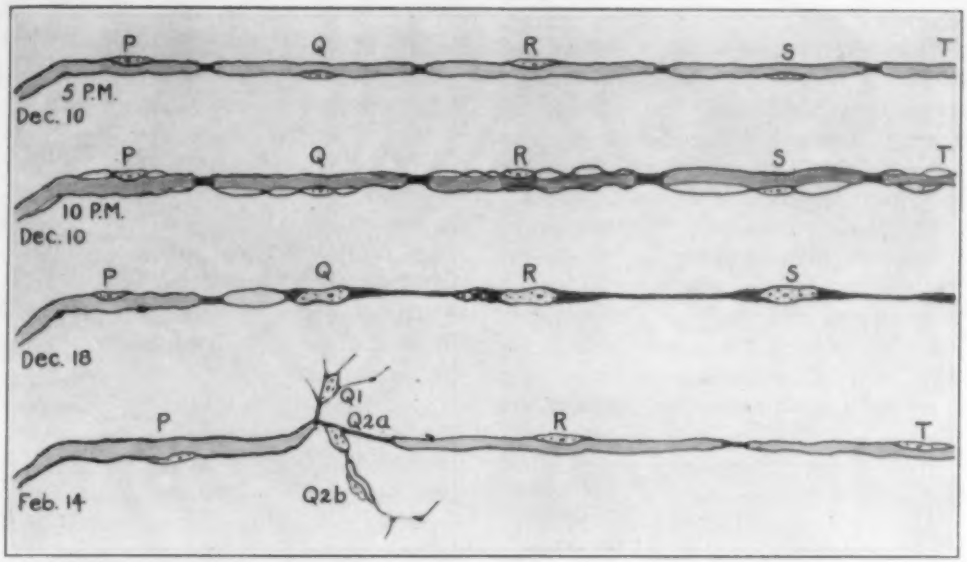


FIG. 3. DEGENERATION OF MYELIN SHEATH SEGMENTS AFTER A SINGLE SEVERE ALCOHOL TREATMENT.

ON DECEMBER 10 A LONG NORMAL MYELINATED NERVE FIBER WAS OBSERVED. FOUR OF THE MYELIN SEGMENTS (P, Q, R AND S) ARE SHOWN. BEYOND S IN THE DIRECTION OF THE END OF THE FIBER ARE 13 MORE SEGMENTS. TO THE LEFT OF P, 5 MORE SEGMENTS WERE VISIBLE. IMMERSION OF THE TADPOLE IN 2.5 PER CENT. ALCOHOL FOR 5 HOURS WAS SUFFICIENT TO STUN IT AND TO INITIATE THE BREAKING UP OF MYELIN SEGMENTS. SEGMENTS P, Q, R, S AND T ALL SHOWED MARKED SWELLING AND VACUOLE FORMATION, AS DID THOSE TO THE LEFT OF P (NOT FIGURED). SEVERAL SEGMENTS TOWARD THE END OF THE FIBER (NOT FIGURED) WERE BREAKING UP. ALL SEGMENTS FROM R TO THE END OF THE FIBER, 15 IN ALL, DEGENERATED; ALL SEGMENTS FROM P IN THE OPPOSITE DIRECTION (PROXIMALLY) RECOVERED. SEGMENT Q DEGENERATED PARTIALLY, BUT THE MYELIN TO THE LEFT JOINED THAT OF SEGMENT P. NEW SEGMENTS WERE FORMED ALONG THE FIBER, TEN IN ALL DEVELOPING TO REPLACE THE LOST ONES. SHEATH CELL Q DIVIDED, AND ONE OF THE DAUGHTER CELLS DIVIDED AGAIN, THREE CELLS THUS ARISING (Q_1, Q_{2a} AND Q_{2b}). NEW COLLATERAL SPROUTS APPEARED IN THE VICINITY. THESE SPROUTS GREW OUT AND ESTABLISHED CONNECTIONS WITH THE SKIN (NOT FIGURED). ON ONE OF THE FIBERS A SMALL NEW MYELIN SEGMENT WAS FORMED AT Q_{2b}.

amputation of a variable length of nerve fiber). Recovery of the endings after normal conditions are restored takes place readily. If the ending is merely swollen it becomes reduced in size. Usually several hours is ample time for such reduction to be effected. If retraction has taken place with a delicate filament left at the end, the neuroplasm flows forward and a growth cone is formed which advances and later becomes transformed into a knob-like ending. If autotomy has taken place, a growth cone may develop and advance along the former route (or a new route) to the skin. Essentially, these reactions are like those already described for growing nerve tips during active regeneration. They are not so conspicuous, however, and rather severe treatments are necessary to bring about the effects. If the treatment with alcohol is mild, the nerve endings exhibit little or no change.

In several cases by careful manipulation I have been able to observe the

autotomy refers to the similar procedure by which a nerve fiber pinches off its terminal portion.

changes in the same set of nerve endings during several successive periods of irritation and recovery. These experiments make it certain that the effects are directly ascribable to alcohol. Successive conditions of retraction and advance have been recorded in dozens of cases.

In the higher animals, all coordinated movements involve more than one nerve cell. Nervous impulses must be transmitted from cell to cell. The place of junction between two nerve cells is the synapse. Unfortunately, however, synapses are not in favorable positions for direct observation. I have not, therefore, succeeded in watching them in the living animal. Nevertheless, the changes in cutaneous nerve endings induced by

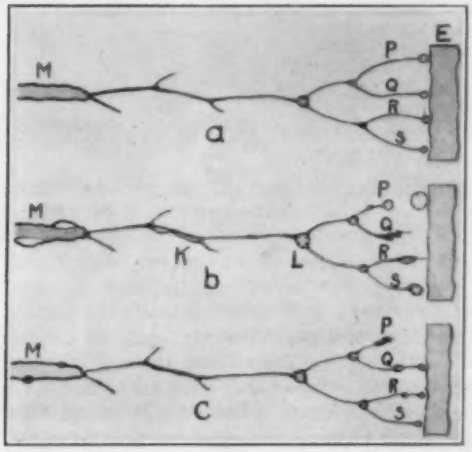


FIG. 5. CHANGES IN THE CUTANEOUS ENDINGS OF A NERVE FIBER DURING ALCOHOLIC INTOXICATION AND RECOVERY.

a, FROM THE TERMINAL MYELIN SEGMENT, M, A BRANCH EMERGES WHICH EXTENDS TO THE SKIN EPIDERMIS, E. FOUR OF THE END BRANCHES (P, Q, R AND S) ARE SHOWN, EACH TERMINATING IN A SMALL END BULB. *b*, AFTER 90 MINUTES OF TREATMENT WITH ALCOHOL (3 PER CENT.) IRRITATION BECOMES MARKED. THE ENDING P SWELLS AND IS CUT OFF. THE ENDING Q RETRACTS SLIGHTLY. THE ENDING R SHOWS RETRACTION OF THE END BULB. THE ENDING S SHOWS NO CHANGE EXCEPT SWELLING OF THE END BULB. *c*, A FEW HOURS LATER THE ENDINGS Q, R AND S HAVE RESUMED APPROXIMATELY THEIR FORMER POSITIONS. A GROWTH CONE HAS DEVELOPED ON P. LATER THIS WILL GROW TO THE SKIN, LOSE THE DELICATE BRUSH-LIKE PSEUDOPODS AND DEVELOP A ROUNDED END BULB.

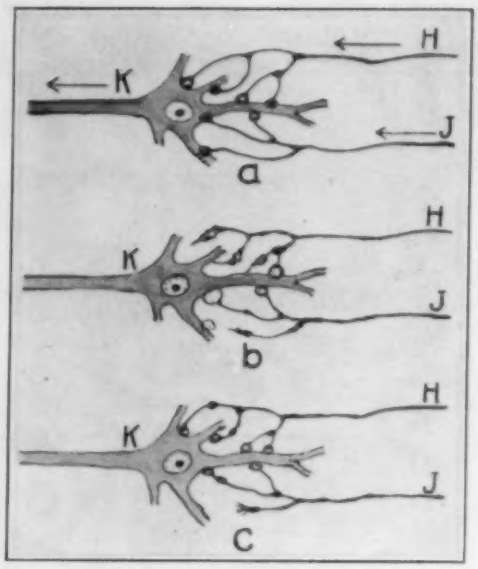


FIG. 6. DIAGRAM TO SHOW HOW THE NERVE ENDINGS AT A SYNAPSE BETWEEN TWO NERVE CELLS MAY BE MODIFIED DURING ALCOHOLIC INTOXICATION AND RECOVERY.

a, THE NERVE BRANCHES H AND J COME FROM NERVE CELLS LOCATED TO THE RIGHT, NOT SHOWN IN THE FIGURE. FIBER H ENDS IN 4 END BULBS ON THE NERVE CELL K; FIBER J ENDS IN 3 END BULBS. NERVOUS IMPULSES PASS ALONG H AND J IN THE DIRECTION INDICATED BY THE ARROWS AT THE RIGHT AND ARE TRANSMITTED ACROSS THE SYNAPSE (THE CONTACT ZONE BETWEEN THE END BULBS AND THE NERVE CELL K) AND THEN PASS ALONG THE AXONE OF K AS INDICATED BY THE ARROW TO THE LEFT. *b*, FOLLOWING TREATMENT WITH ALCOHOL THREE OF THE ENDINGS OF H SHOW DEFINITE RETRACTION OF THE END BULB, AND THE FOURTH SHOWS SWELLING. THE ENDINGS OF J EXHIBIT RESPECTIVELY MARKED SWELLING FOR THE MIDDLE BRANCH, AUTOTOMY OF THE TERMINAL BULB FOR THE BRANCH BELOW AND NO CHANGE FOR THE BRANCH ABOVE. *c*, SEVERAL HOURS AFTER NORMAL CONDITIONS ARE RESTORED THE END BULBS ARE AGAIN IN NORMAL CONTACT WITH CELL K AND THE SWOLLEN ONES ARE REDUCED TO THE USUAL SIZE. A GROWING TIP ON THE LOWEST BRANCH WHICH UNDERWENT AUTOTOMY WILL ADVANCE AND REESTABLISH CONTACT WITH NERVE CELL K.

alcohol afford strong presumptive evidence of what probably takes place at a synapse.

A common type of synapse is that shown in the sketch (Fig. 6, a). Typical nerve endings are shown as they terminate in small end bulbs on the processes and cell body of a nerve cell. During an alcohol treatment of proper strength, swelling and retraction would probably occur (Fig. 6, b). With restoration of normal conditions the endings might again assume approximately their former position (Fig. 6, c).

It is obvious that both swelling and slight retraction of nerve endings at the synapse would probably interfere with perfect transmission of impulses from one nerve cell to the next. Coordination would thus be impaired. If synapses are thus interfered with in the brain there would be trouble with balance, with proper focusing of the eyes, with speech and with the various mental activities.

Effects of alcohol on unmyelinated fibers: Unmyelinated fibers encased with a neurilemma and with sheath cells seem relatively unaffected by alcohol. Except for occasional vacuolation and slight swelling no special changes have been noted, even in tadpoles that at the same time exhibit marked effects in myelinated fibers, growing nerve tips and nerve endings.

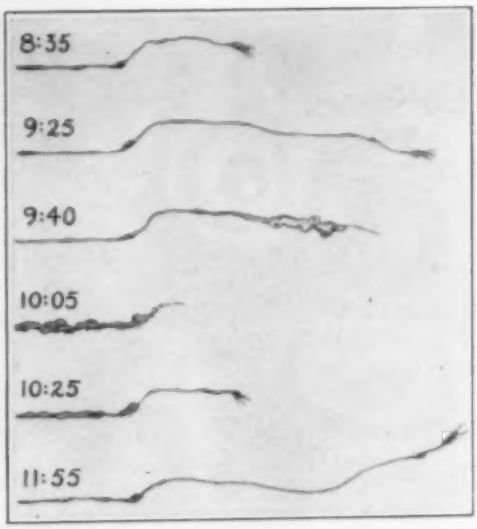


FIG. 4. CHANGES IN THE GROWING TIP OF A REGENERATING NERVE FIBER DURING ALCOHOLIC INTOXICATION AND RECOVERY.

8: 35–9: 25, A NERVE FIBER IN THE REGENERATING TAIL FIN OF A TADPOLE WAS OBSERVED FOR AN HOUR AS IT GREW RAPIDLY THROUGH THE TISSUES. AT 9: 25 THE TADPOLE WAS PLACED IN 2 PER CENT. ALCOHOL. AT 9: 40 DEFINITE RETRACTION WAS APPARENT, WHICH BECAME MORE PRONOUNCED DURING THE NEXT 25 MINUTES. AT 10: 05 THE ANIMAL IN SLIGHTLY DAZED CONDITION WAS REPLACED IN POND WATER. TEN MINUTES LATER THE FIRST SIGNS OF NEW GROWTH WERE VISIBLE AND 20 MINUTES LATER AT 10: 25 THE FIBER TIP WAS ACTIVELY ADVANCING. DURING THE NEXT 90 MINUTES GROWTH AGAIN TOOK PLACE NORMALLY, THOUGH THE FORMER ROUTE WAS NOT FOLLOWED EXACTLY. AT 11: 55 THE FIBER TIP HAD REACHED THE POSITION SHOWN.

DR. GEORGE D. BIRKHOFF

PERKINS PROFESSOR OF MATHEMATICS AND DEAN OF THE FACULTY OF ARTS AND SCIENCES AT HARVARD UNIVERSITY, PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE.

THE PROGRESS OF SCIENCE

THE ATLANTIC CITY MEETING OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

ATLANTIC CITY is unique. In the first place, it is superbly located on a magnificent beach. In the second place, it is admirably adapted for taking care of large conventions. So the association's second Atlantic City meeting, held from December 28 to January 2, was as delightful and as successful as the first one, four years ago.

The attendance at this second meeting was about 4,000. The program included approximately 1,500 addresses and papers. Considerably more than half of these were on biological subjects, and of the remainder nearly one third dealt with agriculture. The physical and medical programs were extensive, and included important contributions.

The programs of the association's meetings reach a far larger audience than the one present when the papers are read. For they are reported by the leading science writers of the country and through them reach all the larger newspapers in the country and also many of those abroad. Through the work of these science writers the people as a whole as well as the members of the association can keep themselves informed regarding the progressive advance of science.

For the information of the members a special lecture was given on science and the American press by Mr. David Dietz, science editor of the Scripps-Howard Newspapers and formerly president of the National Association of Science Writers, which was supplemented by a talk on the history of science and the press by Mr. Watson Davis, director of Science Service. Both Mr. Dietz and Mr. Davis have reported the meetings of the association since 1921.

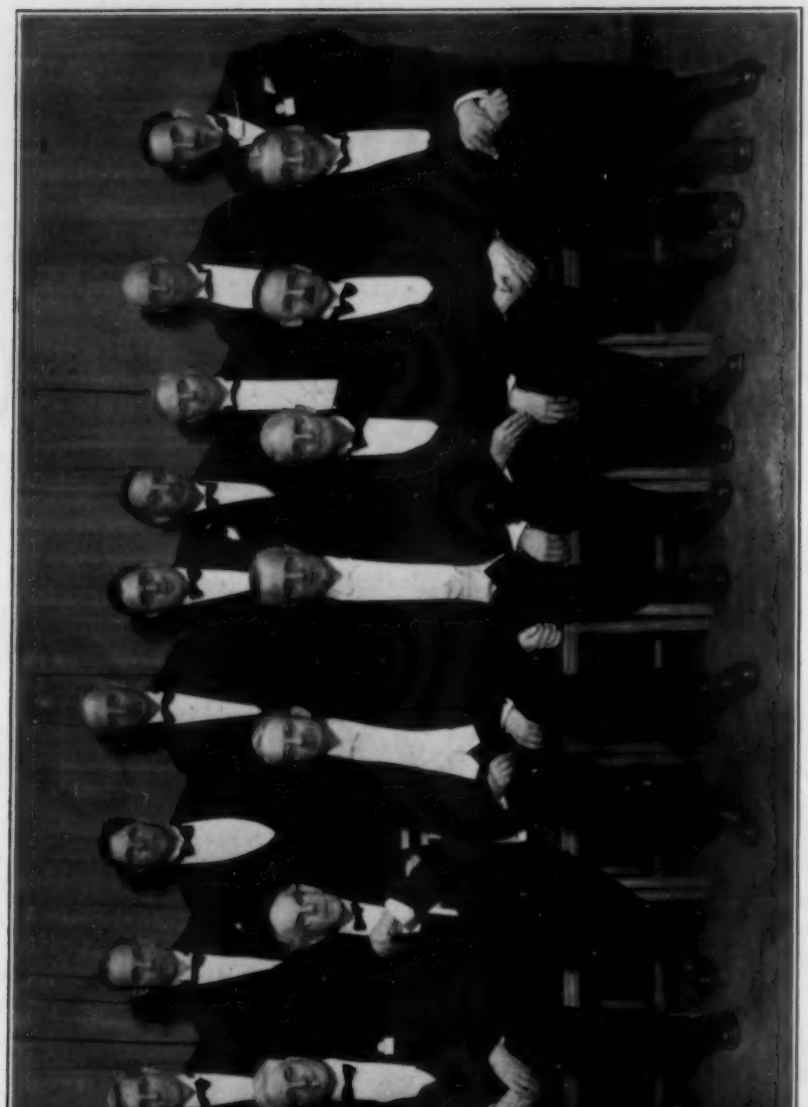
A novelty in this year's program was the setting aside of Friday, January 1, as "Association Day," with special programs arranged for afternoon and evening. Friday morning was mainly devoted to committee and board meetings for the discussion of matters of particular interest to science. Perhaps the most important of these were the meeting of the executive committee of the American Society for the Control of Cancer and the annual Secretaries' Conference.

In addition to these conferences the Ecological Society arranged a field trip to the Cape May peninsula, famous for the apparent anomalies of its fauna and flora, the Oyster Experiment Station and the Witmer Stone Wild Life Sanctuary. The only scheduled meeting was a symposium on New Jersey ferns under the auspices of the American Fern Society.

In the afternoon there were shown a number of unusual moving pictures of exceptional interest, presented by Dr. William Beebe, Perry Burgess, Professor E. M. K. Geiling, Dr. C. E. Turner and Dr. Harold E. Edgerton. Later, Dr. Walter Schiller, of the University of Vienna, gave an address on "Changes and Modifications in the Conception of Carcinoma."

In the evening there was a special private showing of "The Human Adventure," an eight-reel talking picture sketching man's rise from savagery to civilization, produced under the direct scientific supervision of the late Dr. James Henry Breasted by the Oriental Institute of the University of Chicago. The second reel was accompanied by a sound record in the voice of Dr. Breasted.

The association was invited to hold the sessions on Saturday in Philadelphia under the auspices of the American Philosophical Society, one of the oldest, most widely known and most influential of American scientific organizations. This invitation, needless to say, met with an enthusiastic reception. Saturday morning was devoted to a most interest-



OFFICERS OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE PRESIDENT, THE RETIRING PRESIDENT, VICE-PRESIDENTS AND MEMBERS OF THE EXECUTIVE COMMITTEE, ON THE OCCASION OF THE DELIVERY OF THE ADDRESS OF THE RETIRING PRESIDENT, DR. KARL T. COMPTON, PRESIDENT OF THE MASSACHUSETTS INSTITUTE OF TECHNOLOGY. DR. E. G. CONKLIN, EMERITUS PROFESSOR OF BIOLOGY, PRINCETON UNIVERSITY, AND DR. COMPTON ARE SEATED IN THE CENTER. ON THE LATTER'S LEFT IS DR. HENRY B. WARD, PERMANENT SECRETARY OF THE ASSOCIATION.

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DR. C. G. ABBOT, SECRETARY OF THE SMITHSONIAN INSTITUTION, DEMONSTRATING HIS SOLAR ENGINE. FOUR 250-WATT ELECTRIC LIGHTS WERE USED AS A SUBSTITUTE FOR THE SUN AT THE EXHIBITION.

ing program of five papers on viruses and virus diseases. In the afternoon the historic Academy of Natural Sciences and the Franklin Institute were visited. The meetings closed with a most enjoyable tea served at the Franklin Institute.

In connection with the association's program there has been given at each winter meeting for the past thirteen years a prize of $1,000 to the author of a noteworthy paper. The awarding of this prize is made possible through the generosity of a member who prefers to remain anonymous. The recipient of this prize this year was Dr. Wendell Meredith Stanley, of the Rockefeller Institute for Medical Research, at Princeton, New Jersey. His paper was entitled "Chemical Studies on the Virus of Tobacco Mosaic." It is expected that Dr. Stanley will give a more detailed account of his researches at the next winter meeting at Indianapolis.

The exhibitions this year were, as usual, varied, interesting and instructive. Indeed, the entire week could profitably have been spent in studying them. Governmental agencies, both federal and state, universities, commercial firms and individuals combined to assemble a well-rounded science museum. Official participants included the Meteorological Service of Canada, the Smithsonian Institution, the National Bureau of Standards, the Bureau of Chemistry and Soils, the Weather Bureau, the Bureau of Mines, the Federal Bureau of Investigation, the Rural Electrification Administration, the Commonwealth of Virginia, and the Agricultural Experiment Stations of Utah and Nevada. Harvard University and the Massachusetts Institute of Technology joined with the Meteorological Service of Canada, the National Bureau of Standards, the Weather Bureau and leading manufacturers in a display of aerological apparatus.

The exhibits sponsored by the American Society for the Control of Cancer and the National Tuberculosis Association were most instructive to those who had not followed the work of these or-

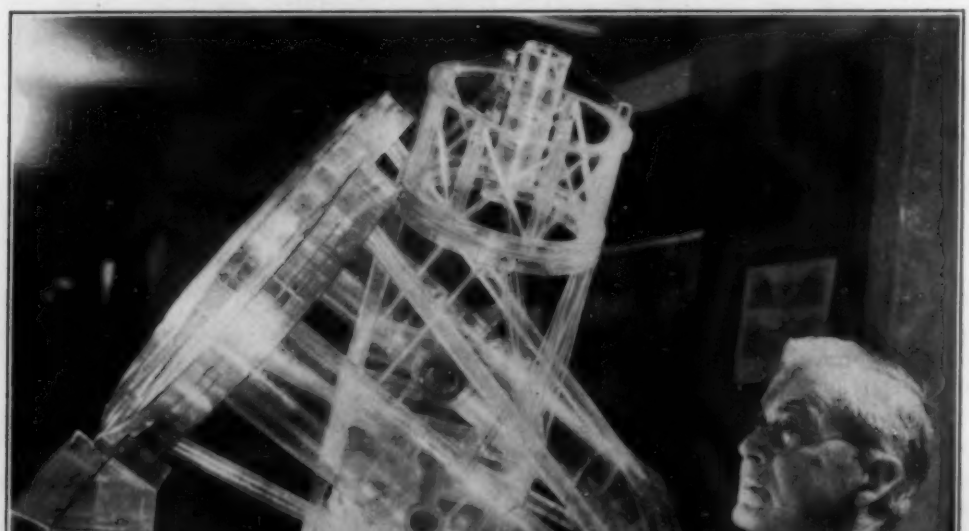
ganizations. The astronomical exhibit showing the work of our American observatories was fascinating, as was that of the cosmic ray apparatus and the charts showing some of the results of recent surveys of the intensity of these rays. The latest and most efficient type of solar engine proved to be of interest to many.

The display of the most recent types of scientific instruments was very instructive. To those among us who remember the physical, chemical and biological laboratories of thirty years ago it was, as usual, a revelation.

As a part of the exhibit the Science Library has become a regular feature, giving just as true a picture of the year's progress in science as do the displays and demonstrations. This year there were shown 523 books, representing practically the complete production for 1936. The association is much indebted to the cooperation of the publishers and others who have made possible this important feature of the annual exhibit.

In addition to the main exhibit in the Municipal Auditorium there were various other exhibits and demonstrations, chiefly of a more or less specialized nature. Of these the one that attracted the largest number of students in other lines, and of laymen, was the diversified display shown by the Entomological Society of America in Haddon Hall.

No meeting of this kind could be successful without the most efficient sort of local cooperation. Efficiency and Atlantic City seem synonymous in so far as the handling of large conventions is concerned. We learned that at the time of our first meeting there in 1932, and our impressions of that time have been confirmed by our recent visit. We are under great obligations to Mr. A. H. Skean,



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DR. G. E. HULL OF DARTMOUTH COLLEGE
EXAMINING THE CELLULOID MODEL OF THE 200-INCH TELESCOPE.

Pictures, Inc.

DR. IRVING LANGMUIR OF THE GENERAL ELECTRIC COMPANY
DEMONSTRATING HIS BUILT-UP FILMS OF PROTEINS AT THE SCIENCE EXHIBITION.

director of the Convention Bureau of Atlantic City, for his interest in science and in our meeting, and for the splendidly cooperative spirit that he showed on all occasions.

AUSTIN H. CLARK

PROFESSOR GEORGE D. BIRKHOFF, PRESIDENT OF THE AMERICAN ASSOCIATION FOR THE ADVANCEMENT OF SCIENCE

THE new president of the American Association for the Advancement of Science, though in appearance a young man, has to his credit actually thirty years of college teaching. Not all on one campus, however, but longest at Harvard University. His educational career has been diversified. Born at Overisel in Michigan on the vernal equinox in 1884, he was graduated from Harvard at twenty-one, and one year later received the A.M. degree. Now began, we may say, his purposeful progress, for he knew what he could do and chose his steps independently. Already he was looked upon as a scholar, for in his junior year he had read an important paper before the American Mathematical Society. This, with extensive additions, he published soon after graduation in Volume 7 of the society's *Transactions*. Studying at the University of Chicago, chiefly under the stimulating influence of the late E. H. Moore, he reached the doctorate in 1907. For the next two years he taught in the University of Wisconsin under the presidency of Van Hise, with such colleagues as Van Vleck, Slichter and Max Mason. Next he is seen at Princeton, at first as a preceptor, where

Dean Fine was assembling the departmental staff whose growth has been so phenomenal. Here his associates included Eisenhart, Thompson, Veblen, Wedderburn, MacNeish. But he had special ends in view, and in 1912 resigned a full professorship to accept an assistant professor's status at Harvard. No doubt early ties were strong, but in particular the close cooperation of Maxime Bocher would be most helpful at that stage in his study of differential equations. There he has remained, in spite of temptations, and advanced to full professorship and quite recently to the rank of research professor and dean of the Faculty of Arts and Sciences. Harvard, Chicago, Madison, Princeton, a summer in California, and back to Cambridge—an instructive range of experience.

These were not merely stations, but centers of mathematical intercourse, and Birkhoff's nature is intensely social. Meantime his field of research and publication has extended. Differential equations, singly and in systems, and particularly those of dynamics and electricity, occupied ten years or more; and he became one of the earliest students and a leading exponent of relativity in this country. His Lowell Lectures (1923) and a later series at the University of California (Los Angeles) were condensed into a little book which is full of clear exposition and criticism of relativity and competing philosophical systems, and is by no means out of date after twelve years—"The Origin, Nature and Influence of Relativity." Reading this, one remembers that Birkhoff's years at Harvard brought contacts with Royce, James, Bergson and Whitehead, reinforcing evidently a native tendency to profound speculation.

A book often gives some measure of the man. Synopsis is impossible, for it is itself a synopsis, but two extracts may give some impression of its nature. First, a cryptic summary: "The meaning of the nature-mind spectrum is a matter of interest. If we are objectivistic in tendency, we call it the Absolute; if subjectivistic, knowledge."

The second, showing the truly catholic attitude of the writer—a confirmed relativist but no fanatic: "The usefulness of an abstraction is relative to its inherent simplicity of structure and its agreement with the facts. The example, the usefulness of the theory of relativity depends on the circumstance that it possesses the same inherent simplicity as the classical theory, while it explains more facts than that theory did."

Not all Birkhoff's philosophizing is so technical. A distinguished preacher told me that much of the obscurity of professional logicians had been cleared up for him by long walks and talks with Birkhoff. Possibly it was this quality of lucidity that won for him the prize of the A.A.A.S. in 1926; it explains much of his popularity as a lecturer.

As a frequent contributor to the *Transactions* of the American Mathematical Society, Birkhoff has done much toward establishing the reputation of that journal at home and abroad. I may mention a short paper in 1913 that attracted immediate attention. Poincaré had enunciated what seemed a beautifully simple theorem on geometric transformations of a plane ring bounded by two circles, but had for years sought in vain for a proof. It bade fair to become as celebrated as Fermat's "Last Theorem." When it came to Birkhoff's notice, however, he saw at once that methods of attack which he had used for several years on other problems would resolve this difficulty, and gave the complete proof in 1913. Not long after, in 1918, the *Transactions* printed his long and elaborate treatment of "Dynamical Systems with Two Degrees of Freedom." This systematized fragmentary theories and reduced the general problem to a finite number of typical or "normal" forms. For this essay he was honored by the society with the first award (1923) of its Bocher prize.

This recognition came five years after the occasion. Meantime, in 1918, the author had been honored by election, at the early age of 34, to the National Academy of Sciences. This early inclusion in that small body of research scientists is rarely achieved; other instances are Newcomb, A. A. Michelson, Theodore W. Richards and George E. Hale. Other honors that have come to him are honorary membership in the Göttingen Scientific Society, the French Academy of Sciences, the Royal Danish Society of Sciences and Letters, the Royal Academy of Lincei and the Academy of Sciences at Bologna. He is one of the few non-Italian fellows of the new Pontifical Academy of Science. Of special prizes, he has received from Venice the Quirini-Stampolia prize, and a Pontifical Academy prize for researches on systems of differential equations. He has just been made an officer of the French Legion of Honor. Honorary degrees have been awarded to him by Brown, Wisconsin and Harvard Universities, and the Universities of Paris and of Poitiers.

As the office of dean marks a growth in educational influence beyond the bounds of a single department, so, his friends believe, does the appearance of his most recent book, not on pure mathematics, entitled "Aesthetic Measure." The next few years will develop its value; as also that of his latest contribution at the recent winter meeting of the A.A.A.S., to the mathematical theory of electricity and gravitation.

Birkhoff's ancestry is from Holland. His physique, tall and lithe, over six feet, with broad shoulders and well-set head, is less slender than that of the boy whose individuality and originality first attracted the notice of the Mathematical Society. It is worth noting that a son, Garrett, shows distinct ability and promise in abstruse mathematics.

This brief sketch may explain in part the election of another modest mathematician to the chair sometime occupied by Chauvenet, Lovering, H. A. Newton, Simon Newcomb and E. H. Moore.

HENRY S. WHITE

AWARD OF THE AMERICAN ASSOCIATION PRIZE TO DR. STANLEY

FOR his paper presenting strong evidence that the virus which causes the infectious mosaic disease of tobacco plants is a non-living crystalline protein, Dr. Wendell Meredith Stanley received the thousand-dollar prize of the American Association for the Advancement of Science. Dr. Stanley gave his paper before the joint session of Section G (Botanical Sciences) with associated societies at Atlantic City on the afternoon of December 29; his work was also presented at a symposium on virus diseases held in Philadelphia on January 2 under the auspices of the association and the American Philosophical Society.

Dr. Stanley's research, indicating that the viruses are giant protein molecules and not invisible living organisms, provides a new scientific approach to the study of all virus diseases—including influenza, infantile paralysis, encephalitis, measles, smallpox, yellow fever and the common cold, as well as many other diseases affecting man, animals and plants.

The first disease-producing virus was discovered forty-five years ago when it was found that the juice from diseased tobacco plants retained its infectious property after passing through a bacterium-proof Chamberland filter. Discovery of a score of other viruses followed rapidly, and they were recognized, along with bacteria, protozoa and fungi, as infectious pathogenic agents. The virus, though capable of increasing indefinitely within susceptible hosts, could not be cultured apart from living cells, nor was it visible under the highest power of the microscope. Pathologists could only speculate as to its nature. One theory held the virus to be a sub-microscopic bacterium-like organism; another considered it a contagious living fluid; a third

regarded it as some kind of inanimate chemical substance. But all efforts to isolate the infectious principle in pure form were futile until Dr. Stanley in 1935 succeeded in obtaining a crystalline protein that possessed the properties of the virus. A series of brilliant investigations have since yielded convincing evidence supporting the view that the protein actually is identical with the virus.

Dr. Stanley was born at Ridgeville, Indiana, on August 16, 1904. He was graduated with the degree of B.S. from Earlham College in 1926. As a graduate student at the University of Illinois he worked in organic chemistry, and took minors in physical chemistry and bacteriology. He received the Ph.D. degree in 1929, and in the same year he married Miss Marian Staples Jay, then a graduate student in chemistry and mathematics. Following work at the University of Illinois as research associate and instructor, he went in 1930 to the University of Munich for a year as international research fellow in chemistry. He was appointed to the staff of the Rockefeller Institute for Medical Research in 1931, and in the following year he joined the newly assembled group of research workers, headed by Dr. L. O. Kunkel, who were investigating fundamental problems of plant pathology at the Princeton laboratories of the institute.

Dr. Stanley has published thirty-eight scientific contributions. His investigations cross the borders of botany, chemistry, physics, zoology, bacteriology and medicine, and include research on organic compounds with bactericidal action toward *Mycobacterium leprae*, the stereochemistry of diphenyl compounds, the isolation of yeast sterols and the preparation of a model that simulates the alga Valonia in trapping potassium. In the Department of Animal and Plant Pathology of the Rockefeller Institute, he has carried on chemical studies of the virus of tobacco mosaic. This work has culminated in the discoveries which were reported in his prize-winning paper. Among significant points brought out by Dr. Stanley are the following:

The pathogenic activity of the crystalline virus protein is 500 times as great as that of the crude juice. Twenty drops of a solution containing 1 pound of the protein in 120 million gallons of water would usually prove infectious, and under the most favorable conditions a solution one-hundred-thousandth as strong (with only 15 molecules per drop) would suffice. Since only a small part of the solution applied to the leaf actually comes in contact with susceptible cells, a very few molecules—or possibly only one molecule—may be capable of producing the disease. Following inoculation the virus protein increases until it makes up about 85 per cent. of all the protein in the diseased tobacco plant.

An unexpected finding was the enormous size of the virus protein molecule, with a weight (as determined by Dr. Svedberg and Dr. Wyckoff) 17,000,000 times that of the hydrogen atom. No other known protein approaches this magnitude.

A series of tests have demonstrated the uniformity and purity of the virus protein. The disease-producing activity, chemical composition, isoelectric point (pH 3.3), molecular weight, x-ray diffraction pattern and optical rotation of the protein obtained from many samples of plant juice were the same. These properties remained constant on recrystallization, on fractional crystallization or on fractionation by adsorption methods.

Assuming the molecule to be spherical, the molecular weight of 17,000,000 corresponds to a protein molecule about 35 millimicrons in diameter—about one fourteenth the diameter of the smallest bacteria. Although the dissolved protein passes a Berkefeld W filter, it is held back by collodion filters through which the smaller molecules of egg albumin can pass. The virus activity can not be separated from the protein by filtration.

The serological properties, as shown by

the precipitin test, were found to be the same for the protein as for the juice from diseased plants. It is significant also that by suitable chemical treatments the virus could be inactivated without destroying its immunological properties.

Different kinds of plants are susceptible to the tobacco-mosaic disease. Dr. Stanley succeeded in isolating the same high-molecular-weight crystallizable protein from diseased Turkish and Burley tobacco and tomato plants—also from such unrelated plants as spinach and phlox; but he has never been able to find this protein in healthy plants.

An interesting suggestion is that the tobacco-mosaic virus may undergo mutation or evolution. Thus four different strains of the virus—including the ordinary, a single-lesion, a yellow and a masked strain—are characterized by four different, though closely related, crystallizable high-molecular-weight proteins.

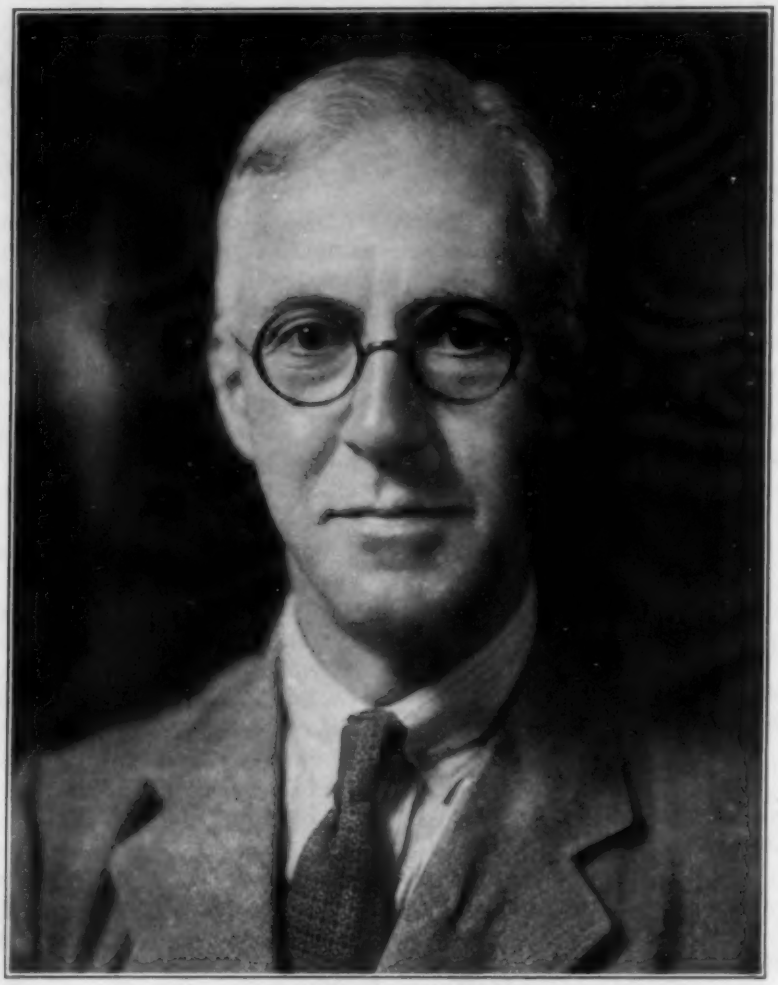
Dr. Stanley has not overlooked the possibility that the crystalline protein which he has isolated may consist chiefly of inert protein plus an amount of active virus agent so small as to escape detection. But in view of the great dilution which the protein will stand and still remain active, it seems highly improbable that the presence of a few tenths of one per cent. of impurity which it may contain could account for the virus activity. Special technique was employed to determine whether the virus activity might be due to an impurity adsorbed on the surface of the protein. The method consisted in the removal of the protein selectively from solution by centrifugal force under conditions that would release any impurity adsorbed on the protein. It has been impossible to separate the virus activity from the protein, but the evidence points to the conclusion that the protein itself is actually the virus.

Although the protein molecule is non-living, it possesses some properties, such as specificity of host range and ability to reproduce and mutate, which are generally regarded as characteristic of living things. The enormous size of this molecule may enable it to possess sufficient organization to endow it with such properties. It is interesting to speculate on some of the implications of the observed facts. Possibly the borderline between the living and the non-living tends to become non-existent in these huge molecules. They may represent a link between the type of organization within the inanimate molecule and the kind of organization within the living cell. In any event, it now appears possible to list protein molecules along with living organisms as infectious disease-producing agents. Infection involves the introduction of a few molecules of a virus protein into a susceptible host. These few molecules appear to have the ability to direct the metabolism of the host so that it produces, not normal protein, but more of the virus protein. Dr. Stanley says that whether one regards this protein as living, as non-living, as a gene, as a super-catalyst, as an organizer or as a pathological protein, its study is most fascinating.

SAM F. TRELEASE,
Secretary of the Section of Botanical Sciences

THE NOBEL PRIZE IN PHYSIOLOGY AND MEDICINE

THE award of the Nobel Prize in physiology and medicine for 1936 to Sir Henry H. Dale of London and Professor Otto Loewi of Graz, because of their investigations of the rôle of acetylcholine as a chemical mediator of nerve impulses, will be recognized widely as well warranted. Admittedly, suggestions that a chemical step intervenes between the nerve impulse and the muscular response had been offered before they began their experiments. In 1904 T. R. Elliott had put forward the bold idea that the reason why the secretion of the adrenal glands, adrenine, duplicates generally throughout the organism the effect of



SIR HENRY H. DALE

sympathetic impulses might be found in the possibility that these impulses normally release adrenine locally on their arrival at the muscle and that this local product acts in all ways like adrenine secreted from the adrenal medulla. Later Dixon had generalized this view. But no experimental proof for it was found.

In 1906 Reid Hunt and Taveau called attention to the remarkable potency of extremely minute quantities of acetylcholine in producing changes in the organism. And shortly before the war Dale discovered the interesting fact that, much as adrenine mimics the action of sympathetic nerve impulses, acetylcholine mimics the action of parasympathetic impulses. The next step was taken when in 1921 Loewi published his impressive and highly significant experiments. They were made in a remarkably simple and direct manner. He prepared two frog hearts, A and B. In the chamber of heart A was introduced normal salt solution. Now the vagus nerves supplied to this heart were stimulated so that the heart was checked in its action or inhibited. After this influence had been exerted for some time Loewi removed the salt solution from heart A and intro-

PROFESSOR OTTO LOEWI

duced it into heart B, which was beating. Thereupon the active heart B was inhibited, much as heart A had been inhibited. The inference was drawn that the vagus impulses on reaching heart A caused the appearance of a chemical substance which not only stopped that heart but which, produced in excess, entered the solution in the cardiac chamber, and, when transferred to the chamber of heart B, had there the same effect as the vagus impulses themselves.

Later studies by Loewi and his assistants brought forth evidence that "vagus substance" is in fact acetylcholine, the material which Dale had proved to be capable of reproducing generally the effects of parasympathetic nerve impulses. Further researches soon demonstrated that other nerves belonging to the parasympathetic category are capable of discharging into perfusates, passed through organs stimulated via parasympathetic fibers, an acetylcholine-like substance, capable of lowering blood pressure, inhibiting the heart, and causing other characteristic alterations.

Investigations conducted by Dale and his collaborators extended the work which he had undertaken in 1914. First,

by finding acetylcholine in the spleen and the human placenta, it was proved to be a natural constituent of the body. Furthermore, evidence accumulated that the curious Vulpian phenomenon—a mysterious slow contraction of denervated voluntary muscles which occurs when autonomic impulses relax the blood vessels of these muscles—could be explained by a diffusion of acetylcholine from the smooth muscles of the vessels to the sensitized striped muscles. Dale also explained an anomaly which has long spoiled the generalization that adrenine mimics sympathetic impulses. As stated above, it is the rule that adrenine does reproduce the effects of these impulses. In man and the cat, however, sweat glands break the rule—pilocarpine, for example. By careful experiments it was shown that the *sympathetic* supply to sweat glands induces a discharge of acetylcholine. On the basis of this observation Dale suggested a more exact classification of autonomic nerves than the division into sympathetic and parasympathetic groups, *i.e.*, a classification into those which produce an adrenine-like substance, adrenergic fibers, and those which produce acetylcholine, the cholinergic.

The most recent and most interesting developments which have come from Dale's group have resulted from extension of the idea of chemical mediation of nerve impulses. A chemical phase appears not only when the impulses arrive at glands and smooth muscle but also when they pass from neuron to neuron in sympathetic ganglia, and from motoneuron to skeletal muscle. Kibjakow, a young Russian, had obtained, in 1932, evidence that a mimetic substance is produced in the superior cervical ganglion by incoming impulses. A perfusate taken during stimulation caused, in Kibjakow's experiments, the same effect as the impulses themselves when it was reperfused through the ganglion. In Dale's laboratory this observation was not confirmed, but clear evidence was obtained that, in fact, when nerves deliver impulses to a sympathetic ganglion, there is produced in the ganglion an appreciable amount of acetylcholine; and that acetylcholine applied to the ganglion has effects similar to those produced by the impulses themselves.

Many years ago T. R. Elliott called attention to the resemblance between the pharmacological evidence of conditions at connections of neurons in ganglia and at connections of nerve fibers with skeletal muscle cells. Last summer an important paper was published by Brown, Dale and Feldberg, announcing that acetylcholine, when introduced directly into the arteries supplying a muscle, produces a strong twitch-like contraction of the muscle. This phenomenon is related to previous evidence which had been obtained by Dale that pure motor impulses, delivered to the muscles of the tongue, liberate acetylcholine. The case becomes, therefore, fairly strong that not only is acetylcholine a mediator of parasympathetic and of some sympathetic impulses—distributed to the heart, the stomach, the intestines, the bladder and salivary and sweat glands—but also that it is an intermediary at the junctions of neurons in ganglia and at the myriads of endings of motoneurons in voluntary muscles.

Obviously a new realm in physiology is disclosed by these important researches, and it will be well to watch for further advances in the attractive realm in which Dale and Loewi have pioneered.

WALTER B. CANNON,
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HARVARD MEDICAL SCHOOL

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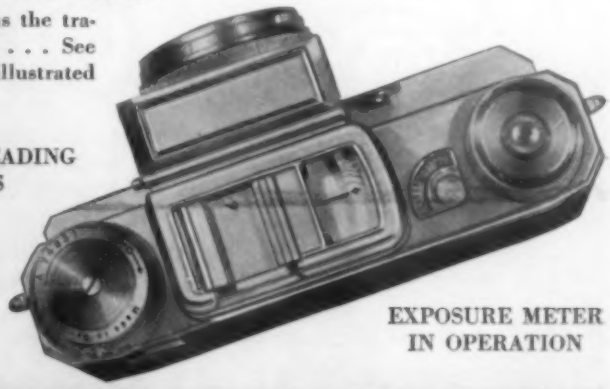

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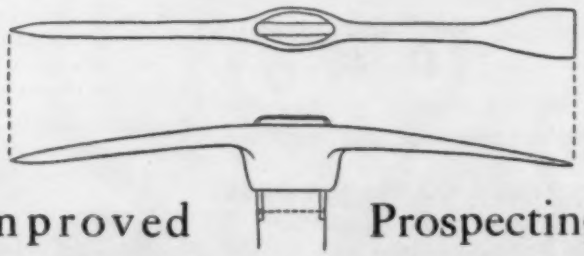

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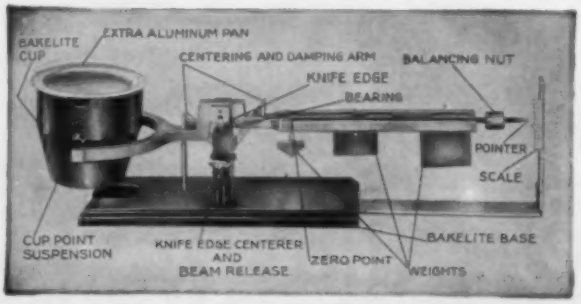

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